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BY WILLIAM NICHOLSON.

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Of Engravings the Subjects are, 1. Station Pointer for determining the precise Situation of a Ship or Vessel from two Horizontal Angles, by Joseph Huddart, Esq., F. R. S. 2. Mr. Dalton's Scheme for shewing the Distribution of the Particles in Mixed Gases. 3. Diagram to shew the Method of constructing and computing the Situation of an Observer from

from the Angular Pofition of Three known Objects. 4. Double Barrel and Winch for encreasing the Power without Diminution of Strength. 5. Screw Press, which by the contrary Action of Two Helical Threads, affords great Increase of Power at the Place of moft extreme Pressure. 6. Pump acting by the Difference of Two Cylinders. 7. Alphabetical System of Writing in the Dark, by Mr. John Gough. 8, 9. Two Plates, Apparatus for fupplying Worm-Tubs, &c. with Water, on the Syphon Principle; By Sir A. N. Edelcrantz. 10, 11. Construction of a fimple Repeater for the Hours and Quarters, By Mr. J. M. Elliot. 12. Diagram to illustrate the Investigations of the Figure of the Earth, by Professor Playfair. 13. Improved Chemical Furnace, by R. Chenevix, Esq. F. R. S. 14. Figures to illustrate an Improvement in Spectacles, by W. H. Wollaston, M. D. F. R. S. 15. New Steam Digester, in which the Heat and Pressure are rendered stationary; by Sir A. N. Edelcrantz. 16. Remarkable Strata of Flint, by Sir Henry Charles Englefield, Bart. F. R. S. 17. Figure to illustrate Dr. Herschel's Theory of the Changes in the Situation of Double Stars. 18. Mr. Hornblower's Apparatus for fweeping Chimnies. 19. Figures to explain the Changes of Double Stars, by Dr. Herschel. 20. Galvanic Apparatus to afford the Maximum of Effect in Combustion, by I. R. I. 21. Hydraulic Engine for raising Fresh Water, by the Discharge of Waste Water, by Mr. Sharples.

*Soho Square, April,* 1804.

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*JANUARY*, 1804.

## ARTICLE I.

*Deſcription and Uſe of the Station Pointer; an Inſtrument for readily aſcertaining the Situation of the Obſerver after having determined the angular Poſition of three known Objects. Conſtructed by* JOSEPH HUDDART, *Eſq. F. R. S. and communicated by him to the Editor.*

Inſtrument for giving a ſhip's place from obs. of two angles of azimuth only.

I HAVE long been deſirous of preſenting my readers with a drawing and account of the inſtrument which forms the ſubject of the preſent paper. Its utility in protracting the ſituation of a veſſel upon a chart from an accurate obſervation by the ſextant of the angular poſition of three known objects on ſhore, inſtead of the uncertain and rough method of bearings by compaſs, is great and obvious; and if any other recommendation could be deſired, it would be that of its repeated application in the hands of a man of ſcience and experience. This we have the ſatisfaction to poſſeſs in the public labours of Mr. Huddart, whoſe maritime ſurveys are too well known and eſteemed to require the ſuffrage of any individual at this day. The very firſt occaſion, when I happened to have the pleaſure of converſing with him, I requeſted the communication of this inſtrument, with which he with great readineſs favoured me, and from which the drawing exhibited in *Plate* I. was made.



It,

[illegible] which being set and duly placed on a chart gives the station of the observer.

It consists of a circle divided into 360° and fitted with three radii, C B, C D and C A; the first of which is fixed at zero and the other two are moveable, and have each a nonius shewing single minutes of a degree. The centre at C is perforated in order to admit the pointer L, which is cylindrical and well fitted to the hole, where it serves to give a correspondent mark upon the chart or paper when in use. I is a magnifier carried by an arm, having its reflector K to throw light on the divisions, and its cylindrical stem H to be inserted in the hollow centre at C. As it must, while in this situation, prevent the introduction of the steel point L, the stem H is itself perforated in order to allow a smaller steel point M to be introduced for the same purpose. The radii A B and D are of brass; but they may be prolonged to an extent that would render them heavy and inconvenient if of metal, by the addition of three rulers of wood, which are attached when required by milled head screws and steady pins, as shewn by breaking the radius A, and also in the side view at G.

The arms D and B are capable of being brought very near each other, and consequently of being set to a very small angle; but A cannot, on account of the noniuses, be brought within ten degrees of D. Whenever, therefore, the left hand angle happens to be small, it becomes necessary to carry the arm A round, until by its situation on the other side of B, it comes to be employed to take the right hand angle, and consequently leaves the left to be taken between D and B.

The pointer A is provided with a clamp, by means of which, that arm can be first secured in its place, and the pointer D is then set without fear of disturbing the situation of the arm first set. It seldom happens that both angles are small in maritime surveying; so that the condition that A cannot be brought very near either of the other arms, has not been found productive of inconvenience.

In *Fig.* 1. *Plate* III. let A, G, and M represent three objects, of which the actual distances from each other are known and projected in a chart. Suppose an observer at H to have measured the angle A H M $=22°.40'$, and the angle M H G $=$ $53°.59'$ it is required to protract the place H by means of the station pointer.

Solution. Set the arms of the instrument to the respective angles, and apply them to the chart so that each point A, M, and

and G, ſhall be found upon the fiducial edge of its reſpective pointer. The center will then be found at H, which may be marked by the ſteel point. Deſcription and uſe of the ſtation pointer.

Rationale of the Proceſs. When an obſerver at H ſees M and A at the angular diſtance of 22°.40′ from each other, he muſt himſelf (by Euclid III. 20.21.) be placed ſomewhere in a circle M A H, in which the line A M is the chord of twice that angle. And ſo likewiſe when he obſerves M and G under the angle 53°. 59′ he muſt be placed in another circle M G H, of which M G is the chord of twice that angle. Conſequently H muſt be at a point common to both circles; namely where they interſect. The more direct the interſection the leſs will the reſult be affected by the errors of obſervation:—when the interſections are very oblique, the reſult will in practice become uncertain; and in the caſe where the two circles coincide throughout, the determination by two angles will be no better than by one; that is to ſay H may be any where in that circle. This laſt caſe occurs when a circle drawn through all the objects does alſo paſs through the ſtation itſelf.

In the figure already referred to H, denotes the weſt garret window of my houſe in Soho-ſquare, and the letters A, G, and M reſpectively denote the ſteeples of the pariſh churches of St. Anne Soho *, St. Giles, and St. Martin, laid down by their difference of latitude and departure from St. Paul's church, obtained from the bearings and diſtances in General Roy's Survey's in the Philoſophical Tranſactions, or the *Account of Operations carried on for accompliſhing a Trigonometrical Survey of England and Wales, &c. publiſhed in quarto by Captain Mudge and Mr. James Dalby. London,* 1799, *with* 22 *plates.*

The table is at pages 194, 195 of the laſt mentioned work. Whence and by obſervation with Hadley's Sextant, the data are: From St. Paul's

St. Anne's bears S. 86°. 9′. 59″ W. 7754 feet.—Southing 518.4—Weſting 7736.6 feet.

St. Martin's S. 74°. 28′. 59″ W. 6748 feet.—Southing 1805.2.—Weſting 6502.1 feet.

St. Giles S. 94°. 36′ 28″ W. 6917 feet.—Northing 55.6—Weſting 6894.7 feet.

* The ſteeple of St. Anne's church has been ſince rebuilt, and ſtands in the ſame latitude, but nine inches farther to the Eaſt than before.

Description and use of the station pointer.

Angle observed between St. Giles's and St. Martin's from Soho Square 22°. 40′½.

Angle observed between St. Martin's and St. Anne's from ditto 53°. 59′.

Construction. Draw the parallel of St. Paul's, and set off the points A, G, and M by the respective differences of latitude and of meridians to represent the churches of St. Anne, St. Giles and St. Martin. Join A and G, the two objects most remote in their bearings, and from the extremities A and G on the side of the line farthest from H, draw the lines Ah, Gh, making angles with A G respectively, equal to the angles observed on contrary sides of the line pointing to the middle object M. Through A,h and G describe a circle; and through M and h draw a right line, which prolonged will cut the circle in H. Join A H, G H, and the angles h A G, M H G, and A G h, M H A will be respectively equal, because standing on the same arcs h G and h A; that is to say, the objects will be seen from H under the observed angles, and consequently H will be the place of the house.

Computation. The numbers in the figure were had by careful construction on a scale of one inch to equal 100 feet, using a beam compass, which divides the inch into 1000 parts. But as the computation may not be unacceptable to beginners in trigonometry, I will here give the process as a conclusion to the present paper.

1. The triangle M A G is known. We have therefore the side A G and two angles given in the triangle A h G, which is thus determined.

2. In the known triangle M A G by subducting the angle A G h from the angle A G M we gain the angle h G M which, with h G, G M, determines the triangle h G M.

3. The angle H h G is (by Euclid I. 32) equal to the sum of the interior opposite angles h G M, G M h. Therefore we have the side h G (by paragraph 1) and the two angles H and h in the triangle H h G. Consequently that triangle is known, and the distance from H, the house, to the object G may be had.

4. In the triangle M G H the angle at G is equal to the sum of the known angles M G h, h G H and the other angle at H and also the side M G are known. Whence the triangle is determined, and the distance from H to the object M may be had.

5. In

5. In the triangle M A H, the angle at M is equal to the difference of the known angles G M A, G M H; and the angle at H, together with the two sides A M, H M are also known. Whence the third side H A or distance from the house to the object A may be found.

6. Lastly the bearing of any of the three sides of the triangle M A G may be had from the known difference of latitude and meridianal distance of its extremities: And this bearing may with ease be then applied to shew the bearings of the lines H A, H M, H G.

W. N.

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## II.

*Experimental Essays on the Constitution of mixed Gases; on the Force of Steam or Vapour from Water and other Liquids in different Temperatures, both in a Torricellian Vacuum and in Air; on Evaporation; and on the Expansion of Gases by Heat. By* JOHN DALTON.

*(Concluded from Page* 273, Vol. VI.*)*

### ESSAY III.

#### ON EVAPORATION.

WHEN a liquid is exposed to the air it is gradually dissipated in it. The process by which this effect is produced we call evaporation. Evaporation defined.

Many philosophers concur in the theory of chemical solution: atmospheric air, it is said, has an affinity for water; it is a menstruum in which water is soluble to a certain degree. It is allowed notwithstanding by all, that each liquid is convertible into an elastic vapour in vacuo, which can subsist independently in any temperature; but as the utmost forces of these vapours are inferior to the pressure of the atmosphere in ordinary temperatures, they are supposed to be incapable of existing in it in the same way as they do in a torricellian vacuum: hence the notion of affinity is induced. According to this theory of evaporation, atmospheric air (and every other species of air for aught that appears) dissolves water, alkohol, ether, acids, and even metals. Water below 212° is chemically combined with the gases; above 212° it assumes a new form, Theory which ascribes evaporation to chemical solution in air—

and

and becomes a diſtinct elaſtic fluid, called *ſteam:* whether water firſt chemically combined with air, and then heated above 212°, is detached from the air or remains with it, the advocates of the theory have not determined.—This theory has always been conſidered as complex and attended with difficulties; ſo much that M. Pictet and others have rejected it, and adopted that which admits of diſtinct elaſtic vapours in the atmoſphere at all temperatures, uncombined with either of the principal conſtituent gaſes; as being much more ſimple and eaſy of explication than the other; though they do not remove the grand objection to it, ariſing from atmoſpheric preſſure. It has however been made to appear in theſe eſſays, I preſume, that the objection to it from preſſure, is itſelf founded upon an ungrounded hypotheſis.

—is obſcure and difficult.

Theory of diſtinct elaſtic vapours in the atmoſphere.

Leaving the theory of evaporation for the preſent, we ſhall proceed to the experiments.

The following poſitions have been eſtabliſhed by others, and need therefore only to be mentioned here.

Poſitions. Different fluids evaporate with different rapidity. Evaporation is as the ſurface; it increaſes with the temperature; and when the air is in motion, and is drier.

1. Some fluids evaporate much more quickly than others.

2. The quantity evaporated is in direct proportion to the ſurface expoſed, all other circumſtances alike.

3. An increaſe of temperature in the liquid is attended with an increaſe of evaporation, not directly proportionable.

4. Evaporation is greater where there is a ſtream of air than where the air is ſtagnant.

5. Evaporation from water is greater the leſs the humidity previouſly exiſting in the atmoſphere, all other circumſtances the ſame.

The objects in view in this eſſay, are,

Objects of the preſent eſſay.

1. To determine the preciſe effect that a variation of temperature has upon the quantity evaporated.

2. To determine the ratio of evaporability of different fluids.

3. To find a rule by which the quantity and effect of previous humidity in the air may be aſcertained.

4. From theſe and other facts to obtain a true theory of evaporation.

### *On the Evaporation of Water at* 212°

Water boiled in a veſſel for a definite time loſt by evaporation a quantity,

I took a ſmall cylindrical veſſel of tin, its diameter 3¼ and depth 2½ inches; and having fixed three pieces of wire to equidiſtant points of the circumference, they were faſtened together

ther at the top, and the extremities bent into a hook, by which the veffel might be fufpended from the end of a balance, &c. This done, the veffel was nearly filled with water, which was then made to boil over a fmall red fire in different circumftances: it was held in the hand and removed nearer to or further from the fire, fo as to be kept juft at the point of ebullition. In this ftate the veffel and water were weighed true to a grain, and the inftant of time noted by a watch; then kept as above at 212° for ten minutes or more and again weighed: and the lofs of water by evaporation, per minute, was thus afcertained. The experiments were repeated feveral times in the fame as well as in different circumftances; and the refults in no inftance differed materially when obtained in the fame circumftances.

The leaft evaporation per minute was 30 grains: this was when the fire, or lamp, was in the middle of a room, the doors and windows fhut, and the air calm. —which was leaft when the furrounding air was ftilleft,

The next degree was 35 grains per minute or thereabouts: this was when the evaporating veffel was over a fmall fire in the ufual fire-place; there being a moderate draught of air, and the room clofe. —and gradually more,

A brifker fire, caufing a ftronger current of air up the chimney, gave from 35 to 40 grains per minute. —the more quickly the air was changed.

When the windows of the room were open, and a ftrong wind prevailed, the draught over the fire was proportionally increafed, and the evaporation was from 40 to 45 grains per minute.

The extremes that have thus been noticed are 30 and 45 grains per minute: but were the experiment tried in the open air in high winds, I am inclined to believe from a comparifon of the obfervations, that an evaporation of 50, 55 or even 60 grains per minute might be obferved.

### *On the Evaporation of Water below* 212°.

I have frequently tried the evaporation at all the temperatures below 212°: it would be tedious to enter into detail of all the experiments, but fhall give the refults at fome remarkable points. In all the high temperatures I ufed the veffel above mentioned, keeping a thermometer in it, by which I could fecure a conftant heat, or at leaft keep it ofcillating within narrow limits. Water at temperatures below 212° loft by evaporation.—

The

The evaporation from water of 180° was from 18 to 22 grains per minute, according to circumftances; or about half of that at 212°.

Quantities which were,

At 164° it was about one third of the quantity at the boiling temperature; or from 10 to 16 grains per minute.

At 152° it was only one fourth of that at boiling; or from 8 to 12 grains, according to circumftances.

The temperature of 144° afford $\frac{1}{5}$ of the effect at boiling; 138° gave $\frac{1}{6}$, &c.

—in every part of the fcale proportional to the force of vapour at that temperature.

Having previoufly to thefe experiments determined the force of aqueous vapour at all the temperatures under 212°, I was naturally led to examine whether the quantity of water evaporated in a given time bore any proportion to the force of vapour of the fame temperature, and was agreeably furprifed to find that they exactly correfponded in every part of the thermometric fcale; thus the forces of vapour at 212°, 180°, 164°, 152°, 144°, and 138° are equal to 30, 15, 10, $7\frac{1}{2}$, 6 and 5 inches of mercury refpectively, and the grains of water evaporated per minute in thofe temperatures were 30, 15, 10, $7\frac{1}{2}$, 6 and 5 alfo; or numbers proportional to thefe. Indeed it fhould be fo from the eftablifhed law of mechanics, that all effects are proportional to the caufes producing them. The atmofphere, it fhould feem, obftructs the diffufion of vapour, which would otherwife be almoft inftantaneous, as in vacuo; but this obftruction is overcome in proportion to the force of the vapour. The obftruction however cannot arife from the *weight* of the atmofphere, as has till now been fuppofed; for then it would effectually prevent any vapour from arifing under 212°: but it is caufed by the *vis inertiæ* of the particles of air; and is fimilar to that which a ftream of water meets with in defcending amongft pebbles.

In low temperatures the evaporation is as the force of vapour at the temperature of the water diminifhed by that exifting in the atmofphere.

The theory of evaporation being thus manifefted from experiments in high temperatures, I found that if it was to be verified by experiments in low temperatures, regard muft be had to the force of vapour actually exifting in the atmofphere at the time. For inftance, if water of 59° were the fubject, the force of vapour of that temperature is $\frac{1}{60}$ of the force at 212°, and one might expect the quantity of evaporation $\frac{1}{60}$ alfo; but if it fhould happen, as it fometimes does in fummer, that an aqueous atmofphere to that amount does already exift, the evaporation, inftead of being $\frac{1}{60}$ of that from boiling water, would

would be nothing at all. On the other hand, if the aqueous atmofphere were lefs than that, fuppofe half of it, correfponding to 39° of heat, then the effective evaporating force would be $\frac{1}{110}$ of that from boiling water; in fhort, the evaporating force muft be univerfally equal to that of the temperature of the water, diminifhed by that already exifting in the atmofphere. In order to find the force of the aqueous atmofphere I ufually take a tall cylindrical glafs jar, dry on the outfide, and fill it with cold fpring water frefh from the well; if dew be immediately formed on the outfide, I pour the water out, let it ftand a while to increafe in heat, dry the outfide of the glafs well with a linen cloth, and then pour the water in again; this operation is to be continued till dew ceafes to be formed, and then the temperature of the water muft be obferved; and oppofite to it in the table (p. 264, Vol. VI.) will be found the force of vapour in the atmofphere. This muft be done in the open air, or at a window; becaufe the air within is generally more humid than that without. Spring water is generally about 50°, and will moftly anfwer the purpofe the three hotteft months in the year: in other feafons an artificial cold mixture is required.—The accuracy of the refult obtained this way I think fcarcely needs to be infifted upon. Glafs, and all other hard fmooth fubftances I have tried, when cooled to a degree below what the furrounding aqueous vapour can fupport, caufe it to be condenfed on their furfaces into water. The degree of cold is ufually from 1 to 10° below the mean heat of the 24 hours; in fummer I have often obferved the point as high as 58° or 59°, correfponding to half an inch of mercury in force, and once or twice have feen it at 62°: in changeable and windy weather it is liable to confiderable fluctuation; but this is not the place to enlarge upon it.

Experiment to determine the force of the atmofpheric vapour.

It is the fame as that of water, which is at the loweft temperature, which condenfes no dew on its containing veffel.

For the purpofe of obferving the evaporation in atmofpheric temperature, I got two light tin veffels, the one fix inches in diameter, and half an inch deep, the other eight inches diameter and $\frac{3}{4}$ inch deep; and made to be fufpended from a balance, like the former one. When any experiment defigned as a teft of the theory was made, a quantity of water was put into one of thefe (generally the fix inch one, which I preferred) the whole was weighed to a grain; then it was placed in an open window or other expofed fituation for ten or fifteen minutes, and again weighed to afcertain the lofs by evaporation;

Narrative of the experiments of evaporation in atmofpheric temperature.

at

at the fame time the temperature of the water was obferved, the force of the aqueous atmofphere afcertained as above, and the ftrength of the current of air noticed. From a great variety of experiments made both in the winter and fummer, and when the evaporating force was ftrong and weak, I have found the refults, entirely conformable with the above theory. The fame quantity is evaporated with the fame evaporating force thus determined, whatever be the temperature of the air, as near as can be judged; but with the fame evaporating force, a ftrong wind will double the effect produced in a ftill atmofphere. Thus, if the aqueous atmofphere be correfpondent to 40° of temperature and the air be 60°, the evaporation is the fame as if the aqueous atmofphere were at 60° of temperature and the air 72°; and in a calm air the evaporation from a veffel of fix inches in diameter in fuch circumftances would be about .9 of a grain per minute, and about 1.8 grains per minute in a very ftrong wind; the different intermediate quantities being regulated folely by the force of the wind.

Account of the table of evaporation.

The following table exhibits the ratios and quantity of water evaporated in each temperature, derived from the preceding theory, and confirmed by experiments, as far as they have been extended. The firft column expreffes the temperature; the fecond, the correfponding force of vapour taken from the preceding table; the other three columns give the number of grains of water that would be evaporated from a furface of fix inches in diameter in the refpective temperatures, on the fuppofition of there being previoufly no aqueous vapour in the atmofphere. Thefe columns prefent the extremes and the mean of evaporation, likely to be noticed, or nearly fuch: for, the firft is calculated upon the fuppofition of 35 grains lofs per minute from the veffel of $3\frac{1}{4}$ inches in diameter; the fecond, 45 and the third 55 grains per minute.

TABLE

## TABLE

Shewing the force of vapour, and the full evaporating force of every degree of temperature from 20° to 85°, expreffed in grains of water that would be raifed per minute from a veffel of fix inches in diameter, fuppofing there were no vapour already in the atmofphere.

Table of the quantities of water evaporated per minute at temperatures between 20° and 85°.

| Temperature. 212° | Force of Vap. inch. 30 | Evaporating Force in Grains. 120 | 154 | 189 |
|---|---|---|---|---|
| 20° | .129 | .52 | .67 | .82 |
| 21 | .134 | .54 | .69 | .85 |
| 22 | .139 | .56 | .71 | .88 |
| 23 | .144 | .58 | .73 | .91 |
| 24 | .150 | .60 | .77 | .94 |
| 25 | .156 | .62 | .79 | .97 |
| 26 | .162 | .65 | .82 | 1. 02 |
| 27 | .168 | .67 | .86 | 1. 05 |
| 28 | .174 | .70 | .90 | 1. 10 |
| 29 | .180 | .72 | .93 | 1. 13 |
| 30 | .186 | .74 | .95 | 1. 17 |
| 31 | .193 | .77 | .99 | 1. 21 |
| 32 | .200 | .80 | 1. 03 | 1. 26 |
| 33 | .207 | .83 | 1 07 | 1. 30 |
| 34 | .214 | .86 | 1. 11 | 1. 35 |
| 35 | .221 | .90 | 1. 14 | 1. 39 |
| 36 | .229 | .92 | 1. 18 | 1. 45 |
| 37 | .237 | .95 | 1. 22 | 1. 49 |
| 38 | .245 | .98 | 1. 26 | 1. 54 |
| 39 | .254 | 1. 02 | 1. 31 | 1. 60 |
| 40 | .263 | 1. 05 | 1. 35 | 1. 65 |
| 41 | .273 | 1. 09 | 1. 40 | 1. 71 |
| 42 | .283 | 1. 13 | 1. 45 | 1. 78 |
| 43 | .294 | 1. 18 | 1. 51 | 1. 85 |
| 44 | .305 | 1. 22 | 1. 57 | 1. 92 |
| 45 | .316 | 1. 26 | 1. 62 | 1. 99 |
| 46 | .327 | 1. 31 | 1. 68 | 2. 06 |
| 47 | .339 | 1. 36 | 1. 75 | 2. 13 |
| 48 | .351 | 1. 40 | 1. 80 | 2. 20 |
| 49 | .363 | 1. 45 | 1. 86 | 2. 28 |
| 50 | .375 | 1. 50 | 1. 92 | 2. 36 |
| 51 | .388 | 1. 55 | 1. 99 | 2. 44 |
| 52 | .401 | 1. 60 | 2. 06 | 2. 51 |
| 53 | .415 | 1. 66 | 2. 13 | 2. 61 |
| 54 | .429 | 1. 71 | 2. 20 | 2. 69 |
| 55 | .443 | 1. 77 | 2. 28 | 2. 78 |
| 56 | .458 | 1. 83 | 2. 35 | 2. 88 |
| 57 | .474 | 1. 90 | 2. 43 | 2. 98 |

*Table continued.*

Table of the quantities of water evaporated per minute at temperatures between 20° and 85°.

| Temperature. 212° | Force of Vap. inch. 30 | Evaporating Force in Grains. 120 | 154 | 189 |
|---|---|---|---|---|
| 58° | .490 | 1. 96 | 2. 52 | 3. 08 |
| 59 | .507 | 2. 03 | 2. 61 | 3. 19 |
| 60 | .524 | 2. 10 | 2. 70 | 3. 30 |
| 61 | .542 | 2. 17 | 2. 79 | 3. 41 |
| 62 | .560 | 2. 24 | 2. 88 | 3. 52 |
| 63 | .578 | 2. 31 | 2. 97 | 3. 63 |
| 64 | .597 | 2. 39 | 3. 07 | 3. 76 |
| 65 | .616 | 2. 46 | 3. 16 | 3. 87 |
| 66 | .635 | 2. 54 | 3. 27 | 3. 99 |
| 67 | .655 | 2. 62 | 3. 37 | 4. 12 |
| 68 | .676 | 2. 70 | 3. 47 | 4. 24 |
| 69 | .698 | 2. 79 | 3. 59 | 4. 38 |
| 70 | .721 | 2. 88 | 3. 70 | 4. 53 |
| 71 | .745 | 2. 98 | 3. 83 | 4. 68 |
| 72 | .770 | 3. 08 | 3. 96 | 4. 84 |
| 73 | .796 | 3. 18 | 4. 09 | 5. 00 |
| 74 | .823 | 3. 29 | 4. 23 | 5. 17 |
| 75 | .851 | 3. 40 | 4. 37 | 5. 34 |
| 76 | .880 | 3. 52 | 4. 52 | 5. 53 |
| 77 | .910 | 3. 65 | 4. 68 | 5. 72 |
| 78 | .940 | 3. 76 | 4. 83 | 5. 91 |
| 79 | .971 | 3. 88 | 4. 99 | 6. 10 |
| 80 | 1. 00 | 4. 00 | 5. 14 | 6. 29 |
| 81 | 1. 04 | 4. 16 | 5. 35 | 6. 54 |
| 82 | 1. 07 | 4. 28 | 5. 50 | 6. 73 |
| 83 | 1. 10 | 4. 40 | 5. 66 | 6. 91 |
| 84 | 1. 14 | 4. 56 | 5. 86 | 7. 17 |
| 85 | 1. 17 | 4. 68 | 6. 07 | 7. 46 |

Use of the table familiarly illustrated.

The use of this table will appear from the following problems:

PROBLEM I.

Having given the temperature at which the aqueous atmosphere begins to be condensed into water, and the temperature of the air, to find the quantity of water that would be evaporated in a minute from a vessel of six inches diameter.

*Solution.* Subtract the grains opposite to the lower temperature from those opposite to the higher one, in the first, second or third column of grains, according to the strength of the wind, and the remainder will be the quantity evaporated in a minute under those circumstances, nearly.

*Example.*

*Example.* Let the point of condenfation be 52°, the temperature of the air 65°, with a moderate breeze. Ufe of the table familiarly illuftrated.

The number oppofite 52° in the fecond column of grains is 2.06, and that oppofite 65° is 3.16; the difference, 1.1 grain, is the evaporation per minute.

PROBLEM II.

Having given the quantity evaporated in a minute, found by experiment, and the temperature of the air, to find the force of the aqueous atmofphere, and the point of condenfation.

*Solution.* Subtract the obferved evaporation from that oppofite the given temperature in the table; and look above for the number neareft to the remainder in the fame column of evaporation, oppofite to which will be found the force of the aqueous atmofphere, and the point at which it begins to be condenfed.

*Example.* Finding the evaporation from a veffel of fix inches in diameter to be 1.7 grain per minute with a brifk wind, air 62°; what is the weight of the aqueous atmofphere, and the temperature at which it begins to be condenfed into water?

The number oppofite 62° in the third column of grains is 3.52, being the whole evaporating force at that temperature in a perfectly dry atmofphere; from which take 1.7 grains, the real evaporating force obferved, and the remainider, 1.82, correfponds, as per table, to the force .294 inches of mercury, the weight of vapour, and to 43° of temperature.*

*Evaporation of Spirits, Ether, &c.*

If the law of evaporation above given apply to water in every part of the fcale of heat, no reafonable doubt can be entertained refpecting its application to other liquids. I have notwithftanding made feveral experiments on others, the refults of which are conformable to the fame law. Some of them follow:— Experiments which fhew that the evaporation of fpirits, ether, &c. follow the fame law as water.

* It may be proper to remind the reader that all the experiments on evaporation are underftood to be made in the open air, or in a window with a current inward; alfo it may be obferved the evaporation in a clofe room is much lefs and is befides irregular, being greater proportionably from a lefs furface, evidently from the ftagnation of the air.

1. Spirit

Experiments which shew that the evaporation of spirits, ether, &c. follow the same law as water.

1. Spirit of wine.—Evaporated from a surface of four inches in diameter, 54 grains in 25 minutes: air 53°; aqueous atmosphere at 49°, and beginning to rain with a moderate breeze. It would proportionally have been 121 grains from a vessel of six inches in diameter. This gives nearly five grains per minute. The same spirit boiled at or near 180°.

Now from the *data*, water of 83° is equivalent in force to spirits of 53°: and it may be seen that the evaporating force of water of 83° is nearly 5 in the first and second columns of grains of the table. It seems probable that the aqueous atmosphere does not diminish the evaporation of spirits as it does that of water.

2. Ether.—1. Put a phial containing ether, and a small tin vessel of $1\frac{3}{4}$ inch diameter into a scale and balanced them exactly: then poured the ether into the evaporating vessel and put the phial into the scale again; took out 40 grains from the opposite scale, and waited till the equilibrium was restored: this was in 8 minutes 6 seconds. The air was 50°, and the ether at first 50°; but it rapidly sunk, as was found by dipping a very small bulbed thermometer into it, to 28°. In a window with a moderate breeze.

——— 2 and 3. Repeated the experiment in the same circumstances, except the evaporating vessel, which was now porcelain, and $2\frac{1}{2}$ inches diameter. Lost 40 grains in 3 minutes. Thermometer sunk from 50 to 30°. The two experiments made this way did not differ above one or two grains.

These results reduced to a vessel of $3\frac{1}{4}$ inches in diameter give

1st. Experiment, loss 17 grains per minute;
2 & 3 ——— — $22\frac{1}{2}$ ———

The reason why the result in the first experiment was something less than in the other two, was evidently owing to the circumstance of its longer duration, by which the ether was the greater part of the time in a low temperature, and consequently evaporated less. The ether used boiled at 102°. At 50° it was therefore in the capacity of water at 160°. But water at 160°, at most loses only 17 or 18 grains per minute, and less 20° below that temperature. At first view therefore it should seem that ether evaporates quicker than the general law assigns. But it must be allowed that the temperature of the *air* has some effect upon evaporation, though it has certainly very

very little. Now ether in the above experiments is acted upon by a current of air of an equal or higher temperature than itself; but water of 160° is usually acted upon by air 100° lower than itself, which is every moment precipitating the vapour formed, and thus obstructing its circulation. This appears to be a sufficient cause for the small difference observed.

Evaporation of mercury, &c.

With respect to mercury, sulphuric acid, muriate of lime, &c. there can be no doubt but they experience a real evaporation like those above; but it must be very small in proportion as their boiling points are high. And it would be difficult to make experiments upon such of these as have an affinity for aqueous vapour; because their acquisition from the aqueous atmosphere would far exceed their loss by evaporation.

The evaporation from ice agrees with the preceding law.

Since writing the above essay, opportunities have occurred to ascertain whether the evaporation from ice is conformable to the same law as that from water. Every one, who has tried the experiment, admits the fact that ice is evaporable. I have lately made several observations on this subject, the results of which, as far as they go, support the conclusion that the general law of evaporation continues the same below the point of congelation as above it. All the experiments were made in the tin vessel above described of six inches in diameter; a quantity of water was suffered to freeze in it, so as to form a circular cake of ice; the vessel and ice were then weighed together, and exposed in the open air for a certain time, after which being again weighed, the loss was found; the force of the aqueous atmosphere was sometimes determined during the experiment by a mixture of pounded ice and salt, in the manner already described.

Experimental results.

| | | Grs. | H. | Grs. | Wind. | Air. |
|---|---|---|---|---|---|---|
| Nov. 5. | In the night lost | 110 in | 9 ; or, | .20 per m. | N. E. brisk. | 28° to 31° |
| —— | at 10 A M. — | 25 in | 1¼; or, | .33 —— | N. E. mod. | 32° |
| —29. | at 1 P. M. — | 24 in | 1¾; or, | .23 —— | calm. | 31° |
| | P. M. — | 84 in | 9¼; or, | .15 —— | ——— | 30° |
| —30. | in the night — | 94 in | 9 ; or, | .17 —— | N. E. mod. | 31° |
| Dec. 19. | P. M. — | 75 in | 8 , or, | .16 —— | N. E. calm. | 26°—28° |
| | In the night — | 33 in | 11 ; or, | .05 —— | calm. | 25° |
| —20. | A. M. — | 21 in | 2 ; or, | .175 —— | W. mod | 31° |

Some of these being made in the night, and of long duration, neither the temperature of the air, nor the force of the aqueous atmosphere could be fairly determined: the second experiment

experiment was made under every favourable circumftance, and the aqueous atmofphere found at 22°. By problem ii, at page 13, it would have been determined at 21½°, ufing the fecond column of grains in the table.

On the fubject of evaporation it may be confidered as unpardonable not to advert to De Sauffure's valuable Effays on Hygrometry.

Experiments of De Sauffure compared with the preceding theory.

That excellent philofopher determined, by a well conceived experiment, that dry air of the temperature of 64° or 66°, imbibed aqueous vapour fo as to increafe its elafticity $\frac{1}{54}$ of the atmofpheric preffure; and that a cubic foot of fuch air required 11 or 12 grains of water to produce the effect. By the table above at page 12 it appears the force of vapour at 61° = .54 = $\frac{1}{54}$ of 29 .5 inches nearly. It is probable this difference is occafioned in part at leaft by the want of perfect drynefs in the air he operated upon, which caufed the increafe of elafticity to be lefs than otherwife. It was, I think, unfortunate that he attached fo much importance to and confidence in his hygrometer; and that he adopted the theory of chymical folution of water in air, contrary to the facts he difcovered, which feemed more reconcileable to the notion of aqueous vapour being a diftinct elaftic fluid. Indeed he is forced to acknowledge in the 1ft. chap. of his Effay on the Theory of Evaporation, that in the ordinary temperature of the atmofphere, aqueous vapour is formed in the firft inftance a diftant elaftic fluid, and *after it has been converted into an elaftic fluid*, it is diffolved by the air; "Je crois qu'il ne la diffout que lorfque l'action du "feu l'a convertie en vapeur elaftique." Now if it can for a moment exift independently under the preffure of the atmofphere, why may it not continue to exift in that ftate?

He confided too much in his hygrometer,

and on that account his tables are inaccurate.

His table of the weight of aqueous vapour in a cubic foot of air at different degrees of the thermometer, being derived from experiments with his hygrometer, except the ftandard one of 66° (15° Reaumur), is far from accurate; and the inaccuracy increafes with the diftance from the ftandard, which, as has been obferved, appears to be nearly correct: in the higher temperatures he makes the water diffolved too little, and in the lower temperatures too much. He fays (§ 93) that the loweft he has feen the hygrometer in the open air, is 40; and that it indicated a reduction of temperature in the air amounting

amounting to 34° .7 (78° of Fahr.) was neceſſary in order to depoſit dew. This obſervation alone is ſufficient to render his hygrometer ſuſpected; for, few who have attended to the formation of dew will admit the probability of ſo large a reduction being neceſſary in any climate or ſeaſon: I believe it rarely requires 40° reduction in temperature in any part of the world to produce the effect.

*Plate* I.* is intended to illuſtrate the author's conception of the conſtitution of the atmoſphere. The different marks or characters of the particles of the gaſes are merely arbitrary, and intended for diſtinction; the ſimple atmoſpheres are given nearly on their real denſities, and the particles are arranged at equal diſtances from each other. In the compound atmoſphere the ſame arrangement is made of each kind of particles as in the ſimple; but the particles of different kinds do not arrange at regular diſtances from each other; becauſe it is ſuppoſed they do not repel each other.

Engraving to ſhew the conſtitution of the atmoſphere.

## III.

*Obſervations on the Cultivation and Growth of Oak Timber. In a Letter from the Rev.* RICHARD YATES, *F. A. S. Chaplain to His Majeſty's Royal Hoſpital at Chelſea, to* CHARLES TAYLOR, *Eſq. Secretary to the Society of Arts* †.

SIR,

TO expatiate upon the vaſt importance of increaſing the growth of oak-timber, ſeems unneceſſary. The national advantages reſulting from this ſource appear to be in general well underſtood; and yet the cultivation and management of this moſt uſeful plant has not hitherto obtained that degree of attention which it moſt certainly merits.

Great importance of the culture of the oak.

Entirely to obviate, or even in ſome meaſure to remove or leſſen, the obſtacles that ſtill continue to impede the planting

* From the ſame Vol. of the Mancheſter Memoirs.—I remind the reader of the references at page 257 of our laſt.

† For which the ſilver medal was voted. Memoirs for 1802, page 80.

of oaks, would therefore be rendering an eſſential ſervice to the nation. The deſire of accompliſhing ſo beneficial a purpoſe, has induced the judicious and public-ſpirited conductors of the Society of Arts to propoſe a premium for "aſcertaining "the beſt method of raiſing oaks:"—in conſequence of which, this paper is ſubmitted to their candid conſideration. And as the ſtatements here made are founded upon a ſedulous and active experience of fifty years, it is preſumed the *ſpirit* and *meaning* of the Society's propoſal may have been obſerved, although it has not been poſſible (in this inſtance) *literally* to fulfil its terms; at leaſt, the very intention of promoting and forwarding the views of ſo enlightened and highly uſeful a Society, may, it is hoped, be accepted as an apology for calling their attention to theſe obſervations.

Cultivation of the oak diſcouraged,

It forms no part of the preſent deſign to enter minutely into the various cauſes that continue to operate in obſtructing the cultivation of oak; as there is one of peculiar magnitude, the conſequences of which are highly detrimental and injurious, and which it is therefore the principal object of this paper to remove.

from a notion of its very ſlow growth,

An opinion is generally prevalent, that the oak is particularly ſlow in its growth, and requires a great number of years before it affords any advantage. This idea too often deters from planting, on account of the very great length of time it is ſuppoſed the land muſt be occupied before any return of valuable produce can be obtained from it, after a conſiderable expence may have been incurred in forming plantations.

which is a miſtake.

This opinion I conſider as entirely founded in error, and to have taken its riſe in a great meaſure from the want of proper management that has hitherto commonly prevailed in the raiſing of oaks: and in this paper I ſhall endeavour ſtrongly to ſtate, that the oak may be rendered very rapid in its growth, and that conſequently land may be employed to great advantage in its cultivation, as a very conſiderable and profitable produce may, in a much ſhorter time than is generally ſuppoſed, be derived from proper parts of an eſtate thus employed.

Oaks are ſuffered to grow (ſlowly) with a ſhort ſtem and large head: *but their ſize may be*

Oak-timber in this country, for the moſt part, appears in trees of a conſiderable extent of head, but ſeldom more than *twenty* or *thirty* feet in ſtem; and this, in many inſtances, the growth of a century. Now, by the courſe of management here

here propoſed, it is conceived that trees, of at leaſt *double this magnitude*, may be obtained in about half that time. *doubled in half the uſual time.*

It is not my intention to attempt a proof of this propoſition by theoretical deductions, but to appeal for its confirmation to the indubitable teſt of fact, which, from the event of repeated trials, impreſſes a conviction, that experience will be found to ſupport and eſtabliſh it in the moſt unequivocal manner.

It would be eaſy to enlarge much on the various qualities of ſoil, the nature and proceſs of vegetation, and the peculiar properties of the oak; but as theſe topics may be found amply and judiciouſly diſcuſſed in many other authors, who have expreſsly treated on theſe ſubjects, I ſhall decline all ſuch ſpeculations: and, with the hope of being more eſſentially uſeful, ſhall confine myſelf to a ſtatement as ſimple and practical as poſſible.

The oak, in the progreſs of its growth, ſpreads numerous roots near the ſurface of the ground, and in an horizontal direction: theſe aſſiſt in ſupporting and preſerving the tree in its poſition, but ſeem to contribute very little to its increaſe and magnitude. The oak appears to derive its chief nutriment and ſtrength from a root that always deſcends at right angles to the horizon, and is called the tap-root. The firſt thing, therefore, to be obſerved is, that upon a judicious attention to this peculiarity, the planter's ſucceſs principally depends; and the neglect of this care is the conſtant ſource of error and diſappointment. In all climates, and upon all ſoils, to preſerve this tap-root from injury, and as much as poſſible to aſſiſt its growth, is a general, and indeed the moſt eſſential principle in the cultivation of oak. With a due regard to this circumſtance, the management of a plantation may be reſolved into the three following practical directions: It is of the utmoſt importance that the perpendicular or tap root be preſerved.

*Previouſly* to planting the acorns, *looſen* the earth intended for their reception, by *deep trenching*. Inſtructions for preparing the ground and planting acorns.

*Never tranſplant*, or in any way diſturb, the ſaplings intended for timber.

Keep the plant carefully *pruned*, till arrived at a proper height.

More fully to elucidate the ſubject, and to prevent the poſſibility of miſapprehenſion, it may be proper to give a more detailed ſtatement.

Oaks for timber are never to be tranfplanted.

In determining on a fpot to form a plantation of oaks for timber, it muft always be recollected that the plants are to remain without removal in their firft, fituation: the clearing and fencing may then be attended to as ufual; and in the courfe of the winter, from September to March, the particular fpots intended for the reception of acorns, may be prepared for that purpofe, by digging a trench about three feet in width, and from three to fix feet in depth, according to the clofenefs and tenacity of the foil. If grafs-ground, the firft fpit fhould be placed at the bottom of the trench; and if more than one trench be neceffary, they fhould be prepared in the fame manner, preferving a diftance of ten yards between each, if it be intended to employ the intermediate fpace in underwood, or for any other purpofe.

Having made a careful felection of acorns that are perfectly found, and in good prefervation, they are to be planted about the middle of March. Draw a drill in the centre of the trench; two inches in depth, if the foil be heavy and loamy; but three inches in a light and fandy earth. In this place the acorns two inches afunder, and cover them carefully with mould. When the plants appear, they muft be weeded by hand in the rows, and the earth of the trench round them cleaned with a hoe, once a month during the fummer. In October infpect the rows, and thin them by pulling up every other plant: attention will of courfe be paid to remove the weak and crooked plants, and leave thofe that are talleft and ftraiteft. On the fecond year, the operation of thinning muft be repeated, at the fame time, and in the fame manner; and, fhould any of the remaining plants have made fide-fhoots ftronger than the general character, they muft be fmoothly cut off with a fharp knife, clofe to the leading ftem. On the third year, the thinning is again to be repeated, and the general pruning commenced, by cutting off clofe to the leading ftem all the fide-fhoots of the firft year; thus leaving the branches of two years to form the head of the following year. The removal of every alternate plant muft be continued yearly, till the trees are about thirty feet apart, at which diftance they may remain for timber. The pruning is to be continued, by removing every year, very fmooth and clofe to the main ftem, one year's growth of fide branches, till the plants are arrived at a ftem of forty, fifty,

or

or fixty feet, and they may then be permitted to run to head without further pruning. Inftructions for cultivating the oak.

The particular arrangement here recommended may be varied according to any peculiarities of fituation, regard being conftantly had to the general and moft important principle of loofening the ground *very deep* previoufly to planting the acorns. By this mode of culture, oaks may be raifed in almoft any foil; but, where it is poffible, a loam or marl is always to be chofen. Oaks thrive much the beft in fuch earth; and, when affifted by *deep trenching* and *judicious pruning*, attain in a few years to an immenfe fize.

Thofe who have been accuftomed to notice the flow growth and ftunted appearance of oak trees, when denied the affiftance of art, and left to themfelves in the common way, would obferve with aftonifhment the vigorous and rapid increafe of plants under the management now pointed out.

The plants thinned out the firft three or four years, though not fit to be depended upon for timber, as tranfplanting generally injures very materially the future growth, may be replanted in the intermediate fpaces between the rows, for the purpofe of being afterwards removed; or they may be ufefully placed in hedges, or other fpare and unoccupied fpots of ground. They fhould be headed down at the time of tranfplanting, as this operation affifts the procefs of nature, in reproducing or remedying any injury the tap-root may have received from the removal: and, if proper attention be given to loofening the foil for their reception, and pruning them as they advance, in moft inftances an adequate profit will be derived from the labour beftowed upon them. After a few years, the produce of the timber-plantation will be found very advantageous. The young trees that are to be removed yearly, will always find a ready market for a variety of purpofes unneceffary here to enumerate. In addition to thefe advantages, if by this treatment of *deep trenching* previous to planting, and *annual careful pruning* during the growth, timber can be produced in about fifty years, of equal quality, and much fuperior in fize, to that which has been above one hundred years growing under improper management, or without the affiftance of cultivation; it will doubtlefs be allowed that a moft beneficial, if not abfolutely the beft poffible method of "raifing oaks," is here pointed out and afcertained.

This

Inftructions for cultivating the oak.

This method of cultivation may perhaps be thought to occafion fo much expenfe in manual labour as to prevent its being generally adopted: it might perhaps be fufficient to obferve, that if the work be conducted with judgment and economy, the future produce would afford ample returns for all neceffary expenditure: it fhould alfo be recollected, that the previous preparation of the ground, and the fubfequent pruning of the plants, are both to be performed at that feafon of the year when a fcarcity of work will enable the planter to obtain affiftance upon eafier terms; with this additional advantage alfo, of providing employment for the labourer at thofe times when the general ftate of agricultural bufinefs renders it difficult for him to find maintenance for himfelf and family without charitable relief.

In 1750, at Ingeftrie in Staffordfhire, the feat of Lord Chetwynd, fome plantations were formed and managed in a great meafure according to the principles here ftated, and the growth of the plants were fo uncommonly rapid, and fo extraordinary, that it could not but attract the notice of all concerned in the conduct of them. The attention to the fubject, then excited, has been the occafion and ground of all the obfervations and experiments made from that time to the prefent, the refult of which is given in this paper.

The extenfive plantations of the late Lord Denbigh, at Newnham Paddox, in Warwickfhire, are well known and much admired. The whole has been conducted with great judgment. About a fquare acre has been employed in raifing oaks upon a plan nearly fimilar to that now propofed, and affords the beft and moft convincing proof of the fuperior utility and efficacy of fuch management. Had the noble Earl been now living, I fhould have been enabled to have laid before the Society fome more detailed particulars: That, however, is now impoffible; this paper, therefore, in its prefent ftate, may perhaps be thought not altogether unworthy of notice, as tending to forward the liberal defigns of the Society, and contributing to the advantage of the public, the author conceiving that the beft method of raifing oaks is afcertained and ftated in it.

Should the Society be in any degree inclined to join in this fentiment, it may perhaps induce them to make fome alteration in the terms of their propofal; as, according to the ftatements

mado

made in this paper, and indeed from what may be feen in every part of the kingdom, in the character and appearance of oaks growing without cultivation, it feems afcertained, that "acorns fet with the fpade or dibble, without digging or "tillage," can never be depended on to form good timber; and even in the moft favourable circumftances of this cafe, the growth will be exceedingly flow and precarious. The fame may be faid of "young plants, previoufly raifed in nurferies, "and tranfplanted;" for if the tap-root be cut, broken, or in any degree injured, which in tranfplanting it is almoft impoffible to avoid, that plant will feldom become a vigorous and flourifhing tree. To form a courfe of experiments on fuch a plant as the oak, is not a very eafy matter. To fulfil explicitly the conditions of the Society would require a great length of time, and would be attended with confiderable expenfe, from which future candidates may in a great meafure be exonerated. The raifing even one acre in the manner here afcertained might be productive of great pecuniary advantage, if the facts and experience detailed in this paper are permitted to prove the inutility of the other two methods, and confequently to remove the neceffity of employing fo much ground upon them, at an expenfe they will never repay.

Inftructions for cultivating the oak.

*Chelfea College, Nov.* 4, 1801.

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## IV.

### *A Firft Memoir on coloured Shadows.* *By Cit.* J. H. HASSENFRATZ.

*(Concluded from Vol. VI. page* 285.*)*

PART THE SECOND.

THOUGH the fhadow produced by the concurrence of the folar light with that of the atmofphere, ufually prefents only that feries of colours contained in the portion of the fpectrum, between green and violet, it is not the fame with the coloured fhadows obferved in the interior of rooms, when feveral lights direct or reflected concur with that of the atmofphere to enlighten the plane on which the fhadow is obferved

Shadows produced by more than two lights, are more various in their tints than thofe before defcribed.

obferved; thefe are capable of fhewing all the colours of the prifm; in fact, we fee red, orange, yellow, green, blue, indigo and violet, more or lefs blackened.

Number of fhadows.

The number of thefe fhadows is almoft always two or three, fometimes four or five; it has even happened that we have diftinguifhed fix.

When thefe fhadows are two in number, the colours they prefent are always thofe called *complementary* colours.

Combination of colours to imitate the folar fpectrum.

The infinite variety of colours obferved in the folar fpectrum may be imperfectly imitated by combining different proportions of the natural colours, red, yellow and blue; and, according to the obfervations of Newton, an artificial white may be formed, by mixing thefe three colours. Thofe colours are denominated *complementary*, which muft be made with one or two of the three colours cited, to produce an artificial white with a given colour.

Complementary colours.

To obtain an artificial white with a red, the yellow and blue muft be mixed with it. The yellow and blue make a green, green is therefore the complementary colour of red, and *vice verfa*.

The complementary colour of the orange formed by yellow and red is the blue.

The complementary colour of yellow is violet.

The term does not fubvert the Newtonian theory of the folar light.

As the complementary colours meet together in a great number of cafes, we have thought it neceffary in the outfet to give them that denomination, which makes no change in the refults, by which Newton proves that the folar ray is compofed of an infinity of homogeneous coloured rays.

If the fpectrum is divided into two parts at the point where the green commences, all the homogeneous rays of the lower part, that is to fay, from the beginning of the red to the beginning of the green, are complementary to thofe of the upper part, that is to fay, from the green to the violet; and that in the natural order of their appearance.

The denomination of complementary colours has no other object but to point out two colours which are found together very frequently; and to avoid, by this fimple medium, the circumlocution which their diftinction would lead to.

Complementary colours of two fhadows;

Whenever two coloured fhadows are feen, they are nearly always complementary to each other; that is to fay, if the fhade of one is red, orange, or yellow, that of the other is green, blue, or violet.

When the number of the ſhadows is three, one of them is almoſt always complementary to the other two. of three;

When there are four ſhadows, two of them are ſometimes complementary to the other two; at other times, one of the ſhadows is complementary to the remaining three. of four.

When there are only two coloured ſhadows, one of them is always of the colour of one of the bodies which reflects the light. If the apartment, in which the ſhadows are ſeen, is commanded by a covering of ſlates which reflects a bluiſh light on the plane where the ſhadows fall, the two colours are blue and orange; if the covering is of tiles, the colours are nearly the ſame. If a meadow or trees reflect light into the apartment, the ſhadows are green and red. If the apartment has coloured hangings which reflect the atmoſpheric light or that of the ſun, the ſhadows will participate of the colours of the lights reflected externally and internally. One of the ſhadows is of the ſame colour as the reflected light; which may be varied by another light,

When the light is reflected through glazed windows upon the plane where the ſhadow is ſeen, as the glaſs is rarely colourleſs, and generally tinged with green or violet, according as iron or manganeſe predominates in its compoſition, the ſhadow participates in the colour communicated to the light in its paſſage through the glaſs. or by paſſing through glaſs,

If the ſurface on which the different lights are received be itſelf coloured, the colours of the ſhadow will be affected by it, and the problem will become more complicated. or by the colour of the ſurface on which the ſhadow is received.

From the obſervations we have collected, it follows naturally, that the coloured ſhadows of the interior of an apartment are capable of yielding all the colours of the ſpectrum; that theſe colours are governed by thoſe of the bodies which reflect the light and illuminate the ſurface on which the coloured ſhadows are obſerved. Thus far theſe obſervations offer nothing which could not have been foreſeen by every perſon who had reflected a little on ſhadows; but that which is remarkable is, that theſe ſhadows are almoſt always accompanied by other ſhadows whoſe colours are complementary to the reflected colours, although no body is diſcovered which is capable of reflecting theſe ſorts of colours. Inferences. Other complementary ſhadows not produced by direct reflection.

Here then is a new ſhadow which ſeems conſtantly to accompany the firſt, whoſe colour has a peculiar affinity for that of the light which produces the primitive ſhadow, and depends on cauſes which we purpoſe to examine in a ſubſequent memoir.

Part

*Part the Third.*

Shadows from two lights are of different colours.

IF in a clear moon-light night, at a time when the lamps are alight in Paris, an opaque black body be brought near a white paper, enlightened both by the moon and a lamp, two diſtinct coloured ſhadows will be perceived, one reddiſh, the other bluiſh.

If a taper, a candle, a lamp, or any other light, be brought near a white paſteboard enlightened by the moon, an opaque body placed at a ſmall diſtance from the paſteboard, forms two coloured ſhadows; that occaſioned by intercepting the light of the moon is reddiſh, and that produced by the other light is blue.

Theſe two ſhadows may be obtained by enlightening a ſurface with the atmoſpheric light and that of a lamp; but this requires that the light of the atmoſphere muſt enter the room where the experiment is made, through a ſmall opening, in order that the ſhadow may be well defined when this light is ſingle.

The effect was not varied by the nature of one combuſtible body made uſe of.

By admitting the light of the atmoſphere into a darkened room through an opening of a decimetre (four inches) in diameter, and illuminating a white paſteboard by this light and that of a lamp, we have found that when the paſteboard was one or two metres (yards) from the opening, two coloured ſhadows were produced; that of the atmoſphere is conſtantly red, and that of the artificial light, blue. We have employed the lights from coals, wood, tapers, candles, alcohol, and even hidrogen gas.

The greateſt part of the ſhadows obtained from two different lights are of two tints, the one reddiſh, the other bluiſh.

The variations in the colour of ſhadows are independent of the intenſity of the lights which illuminate the ſurface. We were careful in all the experiments we are going to relate, to place the light at ſuch diſtances that they ſhould enlighten equally that part of the white paſteboard on which the ſhadows were projected; and to this end we followed the method made uſe of by Bouguer, and which conſiſts in receiving through two holes, on a piece of oiled paper, the rays from two different lights, and removing the ſtrongeſt, or bringing forward the weakeſt, until the two lights were of equal intenſity; always being careful to place the eye at an equal diſtance from the two enlightened points.

The

The experiments were made in a dark room, every part of which was painted black, for the purpose of destroying the colour produced by reflected light. Precautions in making the experiments.

We at first illuminated the pasteboard by the light obtained from the combustion of fish-oil in a common lamp, and by the light of hidrogen gas, produced by the dissolution of zinc in sulphuric acid weakened with water. The shadow produced by intercepting the light of the hidrogen gas, was reddish; that arising from intercepting the light of the lamp, was bluish. Shadows from hidrogen gas and oil;

At the beginning we only used a feeble light from the hidrogen gas. Being fearful that the colour of the shadows might be occasioned by the weak light, we filled a bladder with the gas, and by compressing the bladder strongly, produced a powerful bright light: the plane illuminated by this light and that of the lamp, afforded a similar result; the shadow from the hidrogen gas was reddish, that from the lamp bluish.

On enlightening the pasteboard by the light of a lamp and that of alcohol, the interception of the light of alcohol produced a reddish shadow, and that of the lamp a bluish shadow. from a lamp and alcohol;

By illuminating it with the light of the lamp and that of a candle, the shadow from the candle was black, with a light-reddish tinge; that of the lamp was black, with a light-bluish tinge. from a lamp and a candle;

The pasteboard being illuminated with the light of a lamp and that of a taper, the shadow from the taper was black slightly reddened, and that of the lamp, black a little bluish. from a lamp and a taper;

The pasteboard illuminated, at the same time, by the light of a lamp and that from the combustion of wood, the shadow thrown by the light of the wood was a violaceous blue; that of the lamp a reddish yellow. from a lamp and wood,

The experiment was tried with wood burning with difficulty, and producing little flame; with very dry wood producing much flame; with chips of wood producing a white flame, which reddened that of the lamp. When the surface was enlightened with lights of equal intensity, the coloured shadows offered little variation; that proceeding from the light of the wood was violaceous blue, that of the lamp reddish yellow. in different states.

The first experiment was repeated in glass-houses where wood is burnt. However bright the flame might be which was emitted through the working flues, the shadow produced by it on a surface also illuminated by the light of a lamp, was a violaceous blue, while that from the lamp was reddish yellow. The experiment repeated in a glass-house where wood is burnt;

On

in an iron forge; On illuminating the pasteboard by the light of a lamp and that from the combustion of culm in an iron forge, the shadow arising from the light of the culm was bluish, and that from the lamp reddish.

in a glass-house where culm is burnt; On illuminating the pasteboard by the light of a lamp and that from the culm burnt in the furnaces of a glass-house, where the brilliant flame of the working flues reddened that of a lamp, the result was similar to the preceding: the light from the culm produced a blue shadow, and that of the lamp a reddish shadow.

with a lamp and charcoal. When the pasteboard is illuminated by the light of a lamp and that from the combustion of charcoal, the shadow of the charcoal is blue, and that of the lamp red.

The pasteboard illuminated by the light of a lamp and that of charcoal urged by the large bellows in a chemical furnace, so as to produce a whitish light as well as that violaceous light sometimes observed in the furnaces of a forge and in tall furnaces, constantly afforded the same result. In these two cases the shadow from the light of the charcoal was bluish, and that from the lamp reddish. The identity of the colours of the shadows obtained in these three experiments with charcoal, notwithstanding the colour of the flame was different in each experiment, leads to an opinion that the result depends on the nature of the combustible. Nevertheless, to obtain more probability, we made further observations. We filled a chemical furnace with charcoal, and having covered it with its dome for the purpose of comparing the shadow of the direct light of the violaceous flame arising from the combustion of the charcoal, with that of the light of a lamp, we took notice of the shadows of these two lights at the moment when the flame appeared at the summit of the furnace. For a considerable time we observed that the colours of the shadows were very uncertain: sometimes the shadow of the lamp was blue, and that of the flame red; at other times the shadow of the flame was blue, and that of the lamp red. This alternate change in the colours of the shadows from blue to red, continued until the charcoal, which was in the upper part of the dome, was completely inflamed; then the colours of the two shadows became fixed, that from the flame of the charcoal became bluish, and that from the light of the lamp reddish.

Probability that the colour of the shadows is produced by the nature of the combustible.

Singular change of colour in the shadows.

Repetition of the experiment by burning the

As, from the observations of several persons to whom these results were communicated, we were induced to believe that the

the great intenſity of the light of charcoal obtained by its combuſtion in oxigen gas, might change or make ſome variation in the colour of the ſhadows, we ignited the extremity of a piece of charcoal, and urged its combuſtion by a jet of oxigen gas; the gas was contained in a bladder, which was preſſed by the arm; the light obtained was extremely brilliant. A white parchment, illuminated at the ſame time by this light and that of a lamp, preſented two coloured ſhadows; that from the light of the charcoal was blue, and that from the lamp red. charcoal in oxigen gas, with a ſimilar reſult.

From the experiments we have detailed it reſults, that the ſhadow of the light of hidrogen and of alcohol is reddiſh, when that of the lamp is blue; that the ſhadow of the light of wood, of culm, of charcoal, is bluiſh, and that from the light of the lamp is reddiſh; and as hidrogen and alcohol contain leſs carbon than oil—and dry wood, culm, and charcoal contain leſs hidrogen than oil; it would ſeem that the blue and red colours of the ſhadows of the two artificial lights, bear a relation to each other correſponding with the proportions of theſe two combuſtibles; that the light produced by a ſubſtance in which hidrogen predominates, gives a reddiſh ſhadow, and that produced by a ſubſtance in which carbon predominates, affords a bluiſh ſhadow. Summary of the experiments; whence it is inferred that the colour of the ſhadows depends on the predominancy of hidrogen or carbon.

Although our experiments appear to lead to that concluſion, and we took great care to be certain that the red and blue ſhadows produced by the two lights were independent of the rapidity of the combuſtion and intenſity of the lights; nevertheleſs, we dare not hazard the preſenting this reſult as forming a general law; we prefer waiting until time and new experiments ſhall confirm or deſtroy them.

*Recapitulation.*

FROM the obſervations and experiments related in this memoir, it follows,

*1ſt.* That the ſhadows formed by the direct light of the ſun and that of the atmoſphere, vary from meadow-green to a violet-black, in a gradation through the blue, indigo, and violet; and that this variation depends on the intenſity of the light (of the ſun) compared to that of the atmoſphere.

*2d.* That the ſhadows formed in apartments by the light of the atmoſphere and reflected lights, may preſent all the priſmatic

matic colours, more or lefs changed by black; and that the tints of the fhadows obferved are always complementary to each other.

3*d*. That the fhadows produced on a pafteboard illuminated by artificial lights, are reddifh and bluifh, more or lefs deep; and that very probably the bluifh and reddifh tints of the fhadows depend on the proportions of hidrogen and carbon in the combuftible bodies.

The fubject to be refumed.

We purpofe, if the Inftitute judges this fubject worthy of their attention, to make known, in other memoirs, the experiments we have made to determine the different caufes which contribute to form coloured fhadows in every particular circumftance.

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## V.

*Account of the Preparation of the Hyacinthus non fcriptus, or common Blue-Bell, as a Subftitute for Gum-Arabic. In a Letter from Mr. Thomas Willis of Lime-Street, London, communicated to Charles Taylor Efq. Secretary to the Society of Arts* *.

SIR,

Communication by Mr. Deyeux refpecting gum from the hyacinthus non fcriptus.

I HAVE obferved in the Appendix of the laft Monthly Review, in their Report of the Chemical Annals, No. 115, that mention is made of a letter from Mr. Deyeux, to the authors of the Annales de Chimie; in which an account is given that a gummy fubftance had been difcovered, by Mons. Deyeux, to be contained in the bulb of the plant called hyacinthus non fcriptus; and that the reviewers only fay, "This "article does not at prefent require any further notice than annunciation." As I know, Sir, your benevolent difpofition in promoting and encouraging the arts, the fciences, and manufactures of this kingdom, I beg leave to offer the following obfervations on the above-mentioned article, which I think a fubject proper to lay before your moft excellent Society, and which I have no doubt may become of national utility.

* Extracted from the Society's Memoirs from page 202: The filver medal was adjudged to this Communication.



In

In the year, 1794, whilſt collecting plants in a wood for botanical ſpecimens, I obſerved that the root of the hyacinthus non ſcriptus, the plant commonly called blue-bells, or hare-bells, was extremely mucilaginous; and on taſting it, I diſcovered only a very ſlight pungency. I collected a pound of the bulbs, and, after ſlicing and drying them before a fire, they yielded about four ounces of powder. I thought that by keeping the powder ſome time, the little acridneſs might go off, as it does in the arum-root powder. I taſted it about ſix months after, and found it perfectly inſipid. I concluded it might be rendered uſeful for food or nouriſhment, but at that time purſued the matter no further. Diſcovery of this mucilage by the author in 1794

In the ſpring of 1800, gum-arabic having been a long time very dear, and likely to continue ſo, I thought this mucilaginous root might anſwer ſome of its purpoſes, for external uſe. I therefore procured ſeven pounds and a half of the bulbs, which, when ſliced and dried, produced two pounds of powder. Being ſoon afterwards in company with Mr. Charles Taylor, Secretary to the Society of Arts, &c. I mentioned to him that I had diſcovered a root which grew in great plenty in this kingdom, yielded a very ſtrong mucilage, and which I imagined would anſwer the purpoſe of gum-arabic, in ſome of the manufactories. He ſaid, if I pleaſed, he would ſend ſome of it down to Mancheſter, to be tried by the calico-printers. Propoſal in 1800 to uſe it inſtead of gum-arabic.

Three or four ounces of the powder were given him, and ſent down there: he was informed, upon trial, that it anſwered the purpoſes of fixing the calico-printers colours, equally as well as gum-arabic; and in the ſame proportion, of an ounce and a half of the powder, to four ounces of the mordant. Mr. Taylor received the ſamples of the printed cottons on which it had been uſed. Trial at Mancheſter ſucceſsful.

On the 15th of January, 1801, I furniſhed Mr. Taylor with eight ounces more of the powder; but have not ſince heard the reſult.

As this root can be eaſily procured, and uſed at a leſs price than gum-arabic has been ſold for ſeveral years paſt, I think it may be rendered of great utility; and the Society of Arts, &c. by patronizing it, may be the means of making it a public benefit. The root is cheap.

Care

Inftructions for collecting the roots and fecuring their growth.

Care fhould be taken, and advice given, that the woods fhould not be left deftitute of the roots; and it would be advifeable to offer premiums for the cultivating the roots and offsets, as they are very increafing. By fuch means a conftant fupply may be had, if the roots anfwer the intended purpofes.

I do not prefume to offer any thing refpecting the mode in which the Society may think proper to divulge the difcovery, and promote the ufe of thefe roots; but I imagine, that if the roots are bruifed and ufed frefh, they would anfwer the purpofe better than when dried and powdered; and as it is now a proper time of the year for taking them up, and will continue to be fo for two months, I wifh that the difcovery may be made known as foon as poffible.

I have fent you fpecimens both of the dried roots and powder, that they may be feen at the Society's rooms, by the calico-printers. What I have done have been fcorched a little in drying; but the colour would be much better, if proper care was taken in drying them.

I am, SIR,

Your moft obedient Servant,

*Lime-Street,* THOMAS WILLIS.
*March* 17, 1802.

*To John Baker, Efq. one of the Members of the Society of Arts, &c.*

From the trials made before the committee with this powder, with hot and cold water, from famples of the printed cotton produced which had been printed therewith inftead of gum fenegal, and from experiments made in Manchefter, it appears that the hyacinthus non fcriptus may, in many cafes, be found a ufeful fubftitute for gum-arabic.

A Memoir

# VI.

## *A Memoir on Vinous Fermentation: by Cit.* THENARD *.

THE vinous fermentation has hitherto received more attention than the acetous or putrid, not perhaps that it has any thing in it more remarkable, or more worthy of our conſideration, but becauſe it is in the natural order of things, to take more intereſt and ſet more value on what is of moſt immediate utility.

The vinous fermentation moſt attended to becauſe moſt uſeful.

The date of the diſcovery of the vinous fermentation appears too well eſtabliſhed to be called in queſtion. All hiſtorians agree in ſaying, that the moſt ancient nations knew how to prepare ſpirituous drinks.* It aſcends therefore to the remoteſt times; and, if we pay any credit to the poets, we muſt carry it back to the fabulous ages. Indeed it would be ſurpriſing, if it eſcaped the notice of the earlieſt of men. An ebullition ariſing ſpontaneouſly in a liquid, a whole maſs riſing of itſelf, a ſweet liquor becoming vinous, the change of a saccharine matter into an ardent ſpirit, are all extraordinary things, calculated to ſtrike the attention, and awaken the deſire of tracing them to their firſt cauſes. Accordingly there is no phenomenon more early obſerved, and none that has been the ſubject of more conſideration, or has given birth to more experiments; yet, from one of thoſe contraſts that rarely occur in the annals of ſcience, though it has been the moſt ſtudied, there is not perhaps one, with which we are leſs acquainted. It has been a rock, on which the endeavours of chemiſts in all ages have ſplit. Becher, ſo celebrated for his ſubterranean phyſics, Stahl, the Neſtor of the ancient chemiſtry, Boerhaave, whoſe ideas were ſo great, Rouelle, to whom ſcience is indebted for part of the progreſs it has made during the laſt half century, and Macquer, that maſter in the art of writing, all failed in their attempts to penetrate this myſtery of nature. Lavoiſier, who was capable of ſurmounting the greateſt obſtacles, is the only perſon, who, enlightening the whole ſphere of chemiſtry by his genius, travelled this obſcure path without wandering out of his road. His inquiries into fermentation will ever remain a model of vegetable analyſis. In this, as in every thing elſe

Known to the moſt ancient nations.

Its phenomena excite attention;

and have led to many experiments;

hitherto little known.

All chemiſts have failed to explain it,

except Lavoiſier who alone made ſome progreſs in it.

His inveſtigation is a model of vegetable analyſis.

* Annals de Chimie, June 1803. p. 294.

he did, he obſerved that ſtrictneſs of deduction and accuracy of operation, which are his characteriſtics, and which may be conſidered as the ſource of the ſplendid diſcoveries, that will for ever illuſtrate his name. Still, notwithſtanding the excellence of what he did, he was far from leaving nothing more to be done. Though he ſhed great light on this part of ſcience, the obſcurity with which it was enveloped was ſo great, that it is ſtill ſeen only through a miſt. This is a truth that did not eſcape himſelf; he was well aware that he had only laid open the path to the goal, which no doubt he would have reached, no doubt he would have completed the career he had begun with ſuch ſucceſs, had not death, jealous of his fortune and glory, robbed ſcience of his labours.

But he left it imperfect.

All we know of fermentation in fact is confined to this, that the ſaccharine matter is converted into alcohol and carbonic acid by means of an intermediate ſubſtance. But what is the nature of this ſubſtance? and how does it act on the ſugar? Theſe two grand queſtions form the ſubject of the preſent memoir; queſtions that have been often attempted, without ever having been ſolved. Some have thought, that the fermenting principle reſided in the extractive matter. Others would have it to be in the mucilage, becauſe this more frequently accompanies the extractive matter. Some, again deceived by the preſence of tartar in wine, have imagined they found in this the true ferment; but if, inſtead of confining themſelves to the fermentation of the muſt of grapes, they had turned their attention to that of other juices, in which analyſis cannot diſcover the exiſtence of this ſalt, they certainly would not have fallen into this error. Others, laſtly, inconſiderately adopting all theſe opinions, have aſſerted, that a mixture of theſe different matters preſide as it were over fermentation, effecting the decompoſition of the ſugar, and its converſion into alcohol *

In fermentation ſugar is converted into alcohol and carbonic acid, by means of an intermediate ſubſtance. But what is this? and how does it act? Imagined to be extractive matter, mucilage, tartar, which laſt is not found in all fermenting liquors, or a mixture of all theſe.

Of theſe hypotheſis ſome are evidently falſe; others are ſeducing, and acquire from ſpecious reaſoning ſome degree of probability. But before we admit them, we muſt conſult ex-

* Mr. Thenard here gives a long note, to prove he had not ſeen the prize eſſays written in 1787 and 1788 by Fabroni, from whom ſome perſons at Paris accuſed him of having borrowed, and to ſhow, that Fabroni and he differ eſſentially in this theory; but it is here omitted as irrelevant. T.

perience;

perience; we ought, leaving them all out of the queſtion, to deduce from obſervation the theory, which we are too apt to form beforehand. If the genius of Stahl, inſtead of giving birth to phlogiſton, a being that never exiſted but in the brilliant imagination of that extraordinary man, had interrogated nature by means of experiment more than he did, perhaps it would not have gone aſtray; perhaps Stahl would have diſcovered the truth, and deprived France of the glory of having produced the author of the modern theory of chemiſtry. Such is the courſe I have purſued. Before I formed or adopted any ſyſtem, I obſerved facts, and deduced from them conſequences, which, it appeared to me, muſt guide us to the view of what paſſes in liquors under fermentation. But in a ſubject of ſuch nicety nothing is more eaſy than to deceive ourſelves; and it is particularly for the purpoſe of correcting my notions, if they be not juſt, that I ſubmit to the claſs the reſult of my inquiries *.

Stahl miſled by hypotheſis and neglect of experiment.

Facts obſerved by the author.

My firſt obſervations were made on the juice of gooſeberries, which I had ſtrong reaſons to prefer to any other; its fermentation proceeding with moſt celerity, ſo that it is conſequently beſt calculated to throw light on the cauſes that produce it. All my reſearches were directed at firſt to diſcover the matter, that ſerves as a ferment. It would be making a great ſtep, and almoſt reſolving the problem, or at leaſt diſcovering a number of truths not yet known, to determine the nature of this matter, and to aſcertain whether it be always one and the ſame, or whether there be ſeveral that poſſeſs this property. This important queſtion had ſtruck me long ago: I had even meditated upon it occaſionally, and promiſed myſelf to attempt its ſolution, when the Inſtitute propoſed it as a ſubject of a prize. This was an additional ſtrong motive to my purſuing it. I was far from being diſpoſed to admit ſeveral fermentative principles; every thing led me to believe, that there was but one, and that it was none of thoſe hitherto ſuſpected, ſince in fact neither extractive matter, nor mucilage, nor tartar, &c. acts upon ſugar. But this required poſitive demonſtration; and though I have yet no abſolute proofs of it; as it is by no means demonſtrated, that there are ſeveral, and one is obſerved on all occaſions, this opinion ſeems to me at preſent to deſerve the preference.

He firſt examined gooſeberry juice, as fermenting moſt quickly;

in order to diſcover what ſerves as the ferment,

and whether it be one peculiar ſubſtance, or ſeveral.

He ſuppoſed it to be one and the ſame;

and this, though not demonſtrated, moſt probable.

* This paper was read to the National Inſtitute.

Goofeberry juice contains a flightly glutinous matter, which, feparated by filtration,

Through a linen cloth of clofe texture I preffed out the juice of a killogramme of goofeberries. It was turbid, and held in fufpenfion a flightly glutinous matter, which I feparated by the filter, and wafhed in a large quantity of water. As nothing is to be neglected in experimental fcience, and the moft trifling fact frequently leads to important confequences, I fubjected this matter to a regular examination. I firft mixed it with fugar and water, to fee whether it would caufe them to ferment; and I foon perceived many bubbles of an elaftic fluid to be extricated, which I found to be carbonic acid. The effervefcence continued a week, and at the end of this period the liquor was a pleafant drink, but flightly faccharine; it contained a great deal of alcohol, and might eafily have been miftaken for a wine not yet completely made. It may be fuppofed I redoubled my zeal and attention in the examination of a fubftance, that offered me what I fought. It was natural firft to inquire, whether the whole of it were adapted to decompofe fugar; but a fixth part of its weight being fcarcely able to effect this decompofition, I concluded, that it contained the fermentative principle only in fmall quantity. This I attempted every method in vain to feparate, and obtain apart: nothing therefore remained for me, but to compare it before and after it had ferved to produce fermentation. It was not apparently altered by this procefs; being ftill infipid, infoluble both in water and in alcohol, and affecting neither infufion of litmus nor fyrup of violets: but on diftillation it no longer afforded any trace of volatile alkali. This refult, at which I was not furprifed, and which a fecond experiment confirmed, was neverthelefs a ray of light, that confirmed me in the courfe I fhould purfue. It fhowed me, that the germ of fermentation was of an animal nature, it agreed with the ideas I had before conceived, and gave to my fufpicions an appearance of reality.

occafioned a mixture of fugar and water to ferment,

and become vinous.

But as it fcarcely effects this on fix times its weight; it may be prefumed to contain but a fmall portion of the fermentative principle.

This could not be obtained apart;

not apparently altered by having produced fermentation;

but it ceafed to yield ammonia.

The germ of fermentation is therefore of an animal nature,

not feparable by reagents.

I now examined the juice of goofeberries with great care, to difcover this animal matter, which I confidered already as the true ferment. As it was infoluble by itfelf, it muft be combined with fome fubftance, that held it in folution. All the reagents I employed failing to anfwer the purpofe, I had recourfe to fermentation itfelf, and obferved the phenomena produced by it under all circumftances. I made my experiments on nearly a litre of filtered and perfectly clear juice.

The author therefore had recourfe to fermentation.

 The

The apparatus was placed in a ftove, where the thermometer ftood at 20°: it was not long before a fermentation was evident; a large quantity of carbonic acid was prefently evolved; much froth was formed; the liquor loft its tranfparency, and it even became fo turbid, that a fediment was thrown down, which was more evident as the fermentation approached its end. This fediment was of a yellowifh white colour, glutinous, void of tafte, grew brown on drying in the open air, and became flightly acid. Thrown on redhot coals it burnt in the fame manner as animal fubftances: diftilled in a fmall retort it afforded a confiderable quantity of carbonate of ammonia even cryftallized. It made fugar ferment with extreme promptitude. In fhort it was a fubftance perfectly analogous to the yeaft of beer.

Filtered goofeberry juice, in a heat of 20° foon fermented; evolved much carbonic acid, was covered with froth, became turbid, and depofited a fediment of a yellowifh white, glutinous, infipid, grew brown on drying, and flightly acid. On redhot coals burnt like an animal fubftance, and afforded carbonate of ammonia when diftilled. It caufed fugar to ferment; and is perfectly analogous to yeaft.

I was eager to try whether this phenomenon were general, as it ought to be according to my mode of reafoning: and in fact experience foon taught me, that it was common to all juice in a ftate of fermentation. The muft of grapes, the juice of cherries, pears, peaches, aud apples, and the decoction of barley and of wheat, afford yeaft in their fermentation. The grape juice yielded more than the others, though lefs than that of goofeberries: accordingly it did not ferment fo readily as the latter. The juices of cherries and peaches depofited nearly the fame quantity: thofe of pears and apples afforded very little, which is the reafon why their fermentation is fo flow. I could have wifhed to have had a greater number of fruits at my difpofal, that I might have varied my experiments more: they were fufficient, however, to prove, that, where alcohol is formed, a fediment of yeaft is commonly formed likewife. If they who have any doubt on the fubject remaining will maturely confider the two following experiments, I believe they will find themfelves convinced. I knew that honey diluted with water would gradually be converted into a liquor containing fpirits. Cullen informs us, that the faccharine urine of a diabetic patient undergoes in time the fame change*. Accordingly I fet both of thefe to ferment, and the fediment of yeaft was formed in each.

This fubftance common to all fermenting juices. Grape juice yields moft next to that of goofeberries. The juices of pears and apples afford little, whence they ferment flowly. Yeaft commonly* formed where alcohol is produced; even from honey and water, and diabetic urine.

* It was Dr. Dobfon of Liverpool, who firft found, that diabetic urine changed firft into a vinous liquor, and afterward into an acetous, before it became putrid. T.

It

So that in every ſpirituous fermentation an animal matter is depoſited ſimilar to yeaſt.

It may be laid down, therefore, as demonſtrated, that in every ſpirituous fermentation an animal matter is depoſited, ſimilar in all reſpects to that ariſing from wort, poſſeſſing abſolutely the ſame properties, and in particular that of decompoſing ſugar, and converting it into carbonic acid and ſpirit of wine. This gives riſe to a new queſtion, that naturally preſents itſelf, and ought next to occupy our attention. Is the yeaſt generated in the proceſs of fermentation, or rather was it already formed, and did ſerve as a ferment?

Is this generated in the proceſs of fermentation?

No direct proof, that nature employs it excluſively as a ferment.

I muſt confeſs we have yet no experiments, which directly prove, that nature employs this ſubſtance excluſively to effect the converſion of ſugar into alcohol and carbonic acid. For why ſhould it be depoſited when the fermentation has taken place? It may be ſaid indeed, that the ſugar holds it in ſolution, that it can diſſolve more than is neceſſary for its decompoſition, and that then the exceſs is precipitated. But this theory is feebly ſupported by experiment. Is this a ſufficient reaſon, however, to reject it altogether? Have we not ſeveral inſtances of compounds, that require much time for their formation? and this perhaps is what occurs in the juices of fruits, where the ferment and the ſugar are long in contact with each other. What is certain, or what at leaſt appears probable, is, that, if yeaſt be a product of fermentation, as all liquors that ferment depoſit it, no doubt it owes its origin to one and the ſame ſoluble ſubſtance, from which probably it differs little, and which produces it by its reaction on the ſugar.

If it be a product of fermentation, it probably comes from ſome particular ſubſtance, little changed.

Yeaſt an immediate principle of vegetables.

Whichever of theſe two opinions obtains the preference, after more numerous trials, as I have no doubt, that yeaſt is an immediate principle of vegetables, and in conſequence acts an important part in the phenomena both of art and of nature; as I am likewiſe perſuaded, that, if any other ſubſtance capable of exciting fermentation exiſt, it is in the higheſt degree analogous to yeaſt; that it differs from it very little; that it is compoſed like it of azote, oxigen, carbone, and hidrogen; and laſtly that it has no doubt the ſame mode of acting on ſugar: I ſhall proceed to exhibit with the greateſt care the properties of this matter, which I ſhall henceforward term ferment, and in particular conſider well its action on the ſaccharine principle, in order to eſtabliſh the theory of fermentation. This theory, even ſuppoſing yeaſt not to exiſt already formed, but to be produced in fermenting juices, will ſtill be of utility, and capable of various applications, as will appear below.

and if there be any other ferment, it muſt be ſimilar to it, and compoſed of azote, oxigen, carbone, and hidrogen.

I ſhall

I ſhall not recapitulate the phyſical qualities of ferment, as I have mentioned them ſeveral times already; but I ſhall confine myſelf to its chemical properties, which alone are of eſſential import. It has no taſte. It neither reddens infuſion of litmus, nor changes ſyrup of violets green. The putrid fermentation, which in time it undergoes, is in every reſpect ſimilar to that of animal ſubſtances. By deficcation it loſes three fourths of its weight, and this loſs conſiſts entirely of water. Thus dried it is ſtill capable of exciting fermentation; it is by no means decompoſed, and may be preſerved in this ſtate without alteration for an indefinite time. We may avail ourſelves of this property, to convey it to places at a diſtance from any brewery, or with which it is ſo difficult to keep up an intercourſe, that freſh yeaſt could not be ſent to them, particularly in ſummer, without becoming putrid. Diſtilled in a ſmall retort, and urging the fire ſo far as this would bear, eight parts of ferment left a reſiduum of 2.83 of coal; and I obtained 1.61 of water, 1.31 of oil, and 1.46 of muriate of ammonia on adding muriatic acid. Finally, I collected 0.33 of gas, containing a fifth of its bulk of carbonic acid, and which, when this was ſeparated from it by potaſh, burnt like carbonated hidrogen, and required 1.5 of oxigen for its combuſtion. From this experiment we ſee, that ferment contains in particular a great quantity of carbon.

Chemical properties of *ferment.*

May be preſerved any length of time when dried.

Analyſis.

Water at the temperature of 12° or 15° does not diſſolve $\frac{1}{400}$ of ferment: indeed it diſſolves ſo little, that after ſtanding upon it ſeveral hours, and being well filtered, it has ſcarcely any action upon ſugar. Boiling water occaſions it to undergo a decompoſition, which I ſhall examine in another memoir.

Inſoluble in water.

Boiling water decompoſes it.

Nitric acid, even diluted with water, at eighteen degrees, decompoſes it alſo: it converts it into greaſe; and there is evolved from it at firſt azot mingled with carbonic acid, and afterward nitrous gas at the ſame time.

Nitric acid converts it into fatty matter; and azot with carbonic acid, and nitrous gas are given out.

Potaſh acts upon this ſubſtance in the ſame manner as upon animal matters, and the phenomena in both caſes are perfectly the ſame: with each it forms a ſaponaceous ſubſtance, and a great quantity of ammonia, that is volatilized.

With potaſh it forms ſoap, and ammonia is extricated.

But of all the properties of ferment, no one is ſo remarkable, and at the ſame time ſo uſeful, conſequently no one ſo much deſerves to be ſtudied, as its action upon ſugar: this is intereſting to men in every claſs of ſociety, from the mechanic to the

Its action upon ſugar.

the philofopher; to both by its products, and to the latter becaufe it may be a fertile fource of reflection and of new truths. It is much to be regretted, that Lavoifier did not purfue the inveftigation as he intended, and examine it with that care, which is confpicuous in all his labours. Who was more capable of giving birth to a theory of fermentation, than the author of the modern theory of chemiftry? No doubt he was prevented from doing this by a concurrence of circumftances: and this theory, important as it is to fcience, has hitherto remained vague and hypothetical. Knowing the fermentative principle, it could not avoid naturally making a part of my refearches: if I have not rendered it as clear as I hoped, at leaft the veil with which it was covered is removed, and it refts on reafoning confirmed by experiment.

Of the precipitate one part with five of fugar fermented readily.

Products of carbonic acid and fpirit obtained,

equal to 171.5 of alcohol.

To obtain the folution of this problem, I added together different quantities of ferment and fugar; I obferved in every cafe what became of both; and I confirmed by farther obfervations what the preceding had fuggefted. Sixty grammes (927 grains or nearly one ounce of ferment) not dried, and three hundred grammes (4630 grains) of fugar entered into fermentation readily, the temperature being 15° (centigrade=59° Farenheit.) In four or five days all the faccharine matter had difappeared; 51.5 litres (3041 cubic inches) of carbonic acid had been evolved; the liquor being filtered, and diftilled to two thirds, gave on a fecond rectification 863 grammes of fpirit at 13°. The apparatus was fo contrived, that nothing was loft: the receivers were cooled with common falt and ice. I found by fynthefis, that this quantity of fpirit was equivalent to 171.5 grammes of alcohol at 39°. The refidues left after diftilling the fpirit were poured into difhes and evaporated to drynefs: from the refiduum of the fecond diftillation nothing was obtained, but that of the firft yielded about 12 grammes of a naufeous fubftance, flightly acid, and feebly attracting the moifture of the air. I wifhed to difcover the nature of this acid, but there was too little to afcertain it. Lavoifier fays, it is the acetous. Laftly, of the fixty grammes of ferment, there remained forty grammes of a fubftance, which I believed to be more animalized than the ferment itfelf. I was much furprifed to find, that I obtained from it by diftillation much lefs ammonia. Hence I fufpected, that by mixing it afrefh with fugar, fermentation would again take place, and thus

The refiduum 12 grammes, naufeous and, flightly acid.

Lavoifier fays the acid is the acetous.

Refidue of the ferment.

thus all the azot would disappear. What I had foreseen occurred: at the end of seven days, having filtered the liquor, I obtained as a residuum thirty grammes of a substance, which on distillation gave no trace of volatile alkali. I was persuaded, that the azot was carried off with the carbonic acid gas. To convince myself of this, I collected near 41 litres of the carbonic acid in an inverted vessel filled with solution of caustic potash. The whole was absorbed, which leaves no doubt of its purity.

Produced fermentation afresh, and thus lost all its ammonia.

This was not carried off with the carbonic acid.

What then becomes of the azot? it ought to be found either in the residuum of the ferment, or in the residuum obtained by evaporating the liquor left after distillation, or in the alcohol: but the residuum constitutes only half the ferment employed; the quantity of matter left by 300 grammes of sugar and sixty of ferment amounts only to twelve grammes; and neither of these yields any ammonia on distillation, while ferment affords a great deal. If these observations be just, if I have accurately noticed all the phenomena, if nothing have misled me, we cannot avoid concluding, that the azot must exist in the alcohol. Yet I have sought in vain to discover its presence in this fluid, in ether, and in the acetous acid: on passing these through tubes heated red-hot in the fire, and burning the gasses in Volta's eudiometer by means of the electric spark, we obtain such small quantities, that they are by no means sufficient to decide the question: 24 or 25 parts of gas yield at most one of residuum.

What became of the azot?

It must exist in the alcohol, though not discoverable there.

I have made several other experiments however, which hitherto tend to show, that azot may exist in such a manner as not to be discovered on distillation; consequently that it may be one of the constituent principles of vegetables, though in general when distilled they afford no ammonia. But as I have not repeated these experiments, and they are of such importance, that we cannot be too reserved in announcing them, I purpose to revise and vary them, I will endeavour to appreciate all the circumstances, and if I obtain convincing proofs, I will not delay communicating them to the class.

Azot may exist so as not to be detected by distillation, and therefore may be a constituent principle of vegetables.

However this may turn out, these results afford us sufficient light, to see what passes in the act of fermentation. In this respect I cannot agree in opinion with Lavoisier. I do not believe as he does, that all the carbonic acid formed proceeds from the sugar. How, on such a supposition, can we conceive the

All the carbonic acid formed does not proceed from the sugar.

the ferment to act upon it? I think, that the first portions of acid are owing to a combination of the carbon, of the ferment, and the oxigen of the sugar, and that the ferment gives rise to fermentation by abstracting from the sugar a portion of this principle. To render this idea more clear, I suppose a particle of sugar to be formed of eight parts of oxigen, four of carbon, and one of hidrogen, which is not very remote from the truth, according to the experiments of Lavoisier: one of these eight parts of oxigen will unite with a fourth of a part of the carbon of ferment, and then, the equilibrium between the principles of the sugar being disturbed, they will combine in a different manner, so as to form carbonic acid and alcohol.

Ferment produces fermentation by taking from the sugar a portion of its oxigen.

The ferment has in fact a strong attraction for oxigen; as is proved by its decomposing air with the greatest facility; when acetous and carbonic acids are produced, and the azot is disengaged. If pure air be employed instead of common air, the reaction is still more speedy. I have introduced 15 grammes of ferment into a vessel filled with a litre of pure air; I opened it over quicksilver; a fifth of its bulk was absorbed; the ferment was grown sour, all the oxigen gas had evidently disappeared, and was converted into carbonic acid: the temperature was 15°.

Ferment has a strong attraction for oxigen, as is proved by its decomposing air.

From what has been said we see what becomes of the carbon of the ferment, and we shall learn what may become of its other principles, if we recollect the quantity of products resulting from a given quantity of matter subjected to fermentation, and comparing the nature of the one with the other.

What becomes of the principles of ferment.

From sixty grammes of ferment, and three hundred grammes of sugar we obtained, carbonic acid 95 grammes; pure alcohol 171.5; extractive matter, slightly acid, and containing no azot, 12; residuum of the ferment 40. These 40 grammes still contained 25 of ferment, so that 35 only had been employed for the decomposition of the sugar; and these 35 were reduced to 15 of a white substance* insoluble in water, incapable

Leaves a peculiar substance.

* The yeast deposited by a fermenting juice is seldom pure: in general it contains more or less of this peculiar white substance, which is sometimes so abundant, that the sediment consists of it almost entirely. It is the mixture of these two substances, that constitutes in great part the lees of different wines. All these effects depend on the relative proportions of the ferment and the sugar; if there be little sugar and much ferment, the yeast will be pure; if on

Yeast is seldom pure, containing more or less of this peculiar substance, with which it forms the chief part of wine-lees.

incapable of acting upon sugar, yielding no ammonia on distillation, and leaving a coal, that burns with scarcely any residuum; in short, exhibiting characters that distinguish it from all other substances, and induce me to consider it as a peculiar matter.

It appears then, that ferment takes oxigen from sugar, not only by means of part of its carbon, but also by means of part of its hidrogen; for the quantity of carbon given out by ferment is too small, to be the sole germe of fermentation. The azot disappears, and enters perhaps into the composition of the alcohol; the other principles of the ferment form acetous acid, and a peculiar insoluble white matter, which is precipitated. The acetous acid remains in solution in the liquor left after distillation, with an extractive matter, proceeding no doubt from the sugar, and foreign to it.

The hidrogen of the ferment acts as well as its oxigen in producing fermentation.

It is not probable, that the elements of the sugar, in their reaction upon each other when the equilibrium between them is disturbed, form water: there is very little hidrogen in sugar, and a great deal in alcohol; and besides, on adding together the quentity of carbonic acid, of alcohol, of extractive matter, and of residuum, we have a deficiency of one eleventh only of the matter by which they were produced. This loss must be attributed to the water the sugar contains, and is by no means owing to alcohol's being carried off by the carbonic acid. Of this I have been convinced myself by receiving more than thirty litres of this gas in caustic potash; and by distillation and rectification I obtained only a few grammes of fluid, which had so little taste of spirit, that it was not to be distinguished.*

The elements of the sugar do not form water.

A loss of one eleventh owing to water contained in the sugar.

No alcohol carried off by the carbonic acid.

This

on the contrary there be much sugar and little ferment, the sediment formed will contain but little yeast, and perhaps even none; and then it will consist entirely of the white substance here mentioned, while the supernatant wine will or may be of a saccharine quality, Thus should yeast even be a product of fermentation, it would act an important part in this phenomenon; and if it do not give rise to it in the first place, which is not certain even in this case, at least it continues to give rise to it. This was the reason why I said before, that in all cases nature afforded us numerous applications of the theory I laid down.

This depends on the proportion of the ferment to the sugar.

* Cit. Seguin has laid before the Institute a very different theory of fermentation. He thinks, that in this process water is decomposed,

Seguin's theory of fermentation.

*This theory agrees with the facts, though not yet complete.*

This theory, appears to me ſo much the more probable, becauſe it perfectly accords with facts; and this truth becomes particularly ſtriking when we compare them. I am far, however, from conſidering it as complete. Time no doubt will beſtow on it the perfection it wants; and I hope myſelf ſoon to add to its evidence. I know not whether I ſhall be able to diſcover what becomes of the azot of the ferment: but I ſhall aſcertain without difficulty, whether the reſidual matter obtained, which I conſider as a peculiar ſubſtance, be a product of fermentation, as I believe; whether the ſugar contribute to its formation, which is poſſible; or whether it be ready formed, and merely precipitated, which is contrary to all probability. The experiments, that remove all my doubts on this head, I ſhall relate in a ſecond memoir, in which I ſhall not only elucidate ſuch points in this as may appear equivocal, or at beſt reſting on too ſlight foundations, but I ſhall alſo exhibit all the particulars that reſult from them. Here on the contrary I have avoided them as much as poſſible, and endeavoured to conſider the phenomenon only in a general way.

*What remains to be done.*

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## VII.

*Conjectures reſpecting the Formation of the Ice in the Cavern of Grace-Dieu. By* C. L. CADET, *of the College of Pharmacy*.*

*A cavern 146 feet underground, its entrance 60 feet broad and 80 high, its breadth 135 feet.*

ABOUT ſix or ſeven leagues from Beſançon, near the village of Beaume, and half a league from the abbey of Grace-Dieu, there is a natural grotto, 146 feet below the ſurface of

poſed, that its oxigen unites with the carbon of the ferment, and produces carbonic acid, while its hidrogen combines with the ſugar, and converts it into alcohol. For this theory to be admiſſible, we ſhould obtain more alcohol than there was ſugar; but the fact is, little more than half its weight is produced, and beſides, ſuppoſing all the carbon of the decompoſed ferment to be converted into carbonic acid, at moſt not above a ſixth part of the quantity actually produced would be formed, as we may eaſily convince ourſelves from calculations already eſtabliſhed. It may likewiſe be objected to this theory, that ſugar contains a great deal of oxigen, and alcohol very little.

* *Annales de Chimie,* No. 134, Feb. 1803, p. 160.

the

the plain. The entrance of it is 60 feet wide, and about 80 high. Within its greateſt breadth is 135 feet. The ſtone that forms the rock is calcareous carbonate, in part lamillated.— This grotto is diſtinguiſhed from all others by a very ſingular phenomenon: in ſummer ice is conſtantly formed in it in large quantities, and this ice diminiſhes at the approach of winter.

Ice is formed in it in ſummer, and diminiſhes in winter.

As this natural ice-houſe has been accurately deſcribed by Mr. de Croiſmare in the Encyclopédie, and ſeveral other deſcriptions agreeing with his are to be found in the Memoirs of the Academy of Sciences for 1712, thoſe of the *Savans Etrangers* for 1743, the works of Mr. le Cat and Ravier, and Madame Ganthier's Tour of a French Lady in Switzerland, I ſhall confine myſelf to a few obſervations, that have eſcaped the notice of many of the learned who have viſited it, and combine to explain the formation of the ice in this cavern.

Deſcribed by various writers.

I viſited it about the end of September, 1791, and then found but little ice. My guide, who was accuſtomed to conduct travellers to it, informed me, that a month before the ice was three times the bulk of what I ſaw. The perſon who ſells refreſhments at Chammars, a public garden at Beſançon, told me the ſame. He ſaid, that when the winter had proved mild, ſo that they were not able to fill the ice-houſes in the city, he ſent in ſearch of ice to the cavern of Grace-Dieu, and that he choſe for this the warmeſt days, *becauſe then there was moſt ice* in the cavern. Meſſrs. de Croiſmare, le Cat, and Ravier, in their accounts, equally atteſt, that in ſummer the ice is more abundant. The inhabitants whom I met with on the mountain, and of whom I enquired concerning this fact, all told the ſame thing. Conſequently, though the variation of the ice is queſtioned by Mr. Dunod, quoted in the work of Madame Ganthier, I readily admit the exiſtence of this phenomenon, particularly as I think I can explain it.

In September, 1791, it had but little ice.

A month before it had thrice the quantity.

Ice there in mild winters.

By ſeveral authorities the ice is moſt plentiful in ſummer.

The rock that forms the roof of this grotto is lower than all the neighbouring mountains, and even than the ſurface of the plains. Its temperature therefore ought to be mild, like that of all ſubterraneous caverns, except at the moment when ice is accumulated there in abundance. Ten waggons would have been ſufficient to carry away all I ſaw, and the air of the grotto did not feel colder to me than the open air. The authors I have quoted ſay, that the cold in it is below 0. I had no thermometer, but I can affirm that this aſſertion is at leaſt queſtionable.

The rock that forms the roof low.

The air in it does not feel cold.

Defcription of the cavern.

A juft idea of the fituation of the cavern is fufficient to fhew in what manner the water percolates the ftrata of rock, and falls perpetually from the roof. The cavern is furrounded by thickly wooded mountains, and from one of the neareft rocks rufhes a torrent, that forms three cafcades, and turns feveral mills. But what is remarkable is, that the water filtering into the cavern does not freeze as it falls: it is received into a bafin of two or three feet diameter formed in the ice, and remains fluid on a level with the fides of the bafin. I drank fome of this water, which did not feem to me very cold. It was perfectly clear and good.

Clear water burfts through the rocks and falls into a bafon of ice.

The grotto is covered by bufhy trees.

I obferved, that the grotto is not only furrounded by wood on all fides, but that it is covered by a number of the moft beautiful beech, and bufhy chefnut trees. Thefe deny all accefs to the rays of the fun, and always preferve a cool damp air round the cavern. This fhade however would not be fufficient to produce an icy air, but the leaves of thefe trees are fo many organs of perfpiration, that pour into the atmofphere the moifture which exhales from their veffels, and that has been pumped up by their roots. This evaporating water cannot pafs from the ftate of a fluid to that of a gas, without abforbing a large quantity of caloric: the ground therefore is continually cooled by the evaporation the trees produce. In winter the warmth of the fun no longer draws up moifture; the leaves, which botanifts confider as real lungs, have difcontinued their organic action; and the fame evaporation no longer exifts. The temperature of the cavern then changes, and approaches that of fubterranean places and grottos in general. Several facts ferve to confirm this theory, and to prove, that the cold produced in the cavern of Grace-Dieu is entirely owing to the fpeedy evaporation of the furrounding moifture.

The ground is cooled by the evaporation in fummer,

which does not take place in winter.

The trees over the entrance being cut down, lefs ice was formed.

Mr. Ravier, fecretary to the bifhop of Bellay, fays, that in 1724, the entrance of the grotto was overfhadowed by large bufhy trees, that thefe were cut down, and that the quantity of ice afterwards formed has been lefs.

The entrance was partly clofed up by a wall,

He alfo reports, that Mr. de Vanolles, intendant of the province, defirous of preferving this natural curiofity, clofed up the grotto by a wall twenty feet high, with a fmall door, the key of which was entrufted to the bailiffs of the village, with a charge, that, no one fhould be fuffered to enter for the purpofe of carrying away the ice. But this precaution produced

by which the ice was diminifhed.



an

an unexpected effect: the ice perceptibly diminiſhed, and they were obliged to pull down the wall, which was in ſome meaſure an obſtacle to the evaporation. I ſaw the remains of this wall, and the inhabitants of Grace Dieu confirmed to me the fact mentioned by Mr. Ravier.

The formation of the ice in this curious grotto, therefore, may be explained, I believe, according to the theory of the jars that ſerve to cool the water they contain in hot countries, by permitting part of the fluid to tranſude through their porous ſides.

This ice ſtated to be formed in the ſame way as water in porous jars is cooled in hot countries by the evaporation from the outſide.

As no natural philoſopher has yet attempted to explain this intereſting phenomenon, I have flattered myſelf that I might be indulged in a conjecture, founded on phyſical principles fully eſtabliſhed by experience.

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## VIII.

*Obſervations on the Structure of the Tongue: illuſtrated by Caſes in which a Portion of that Organ has been removed by Ligature. By* EVERARD HOME, *Eſq. F. R. S.**

*(Concluded from* VOL. VI. *Page 379.)*

ENCOURAGED by the reſult of this caſe, I was led to perform a ſimilar operation upon a perſon at a more advanced period of life.

Another caſe treated in the ſame manner,

Margaret Dalton, 40 years of age, was admitted into St. George's hoſpital, on the 25th of December, 1801, on account of a tumour, the ſize of a pea, ſituated on the right ſide of the tongue, near its edge. The hiſtory of the caſe was as follows. A ſmall pimple appeared, and gradually increaſed, without pain; the only inconvenience was, that it affected her ſpeech, and, when bruiſed by the teeth, bled freely.

The operation was performed on the 11th of January, 1802, in exactly the ſame manner as has been already deſcribed. It produced a conſiderable degree of ſalivation, which was extremely troubleſome, (much more ſo than the pain the ligatures produced,) and continued till the ſlough came away. The ligature neareſt the root of the tongue ſeparated on the 6th day; the other on the 7th; and, in three days after the ſeparation of the ſecond ligature, the wound was completely ſkinned over.

with complete ſucceſs.

A

A third cafe.

A third cafe of this kind came under my obfervation, in which there was a fmall tumour in the fubftance of the tongue, about the fize of a pea, which gave me the idea of its being of that kind which might terminate in cancer. The patient was a gentleman of about 41 years of age. Upon examining the tumour, I told him of my alarm refpecting its nature; and at the fame time added, that I was very ready to remove it, fhould it be the opinion of other practitioners that fuch a ftep was adviseable; and my experience in two former cafes led me to believe it might be done with fafety. I therefore advifed him to confult other medical practitioners of reputation, and acquaint me with their opinion. Mr. Cline was confulted, and his opinion coincided with mine; which made the patient decide upon having the tumour removed.

The operation, and hiftory of its progrefs and fuccefsful termination.

The operation was performed on the 28th of December, 1802. The needle pierced the tongue an inch beyond the tip, a little to the right of the middle line of the tongue; and the fpace between the two ligatures, when they were tied at the circumference of the tongue, was fully an inch. The tongue was thick; and the mafs included by the ligatures was fuch as to make it difficult to comprefs it. The operation gave confiderable pain, of a numbing kind. Immediately after the operation, the part included became dark coloured, particularly towards the middle line of the tongue. A falivation took place. The next day, the pain and falivation were great, and the patient could not fwallow; but, on the day following, he could take broth, negus, and other fluids.

On the 6th day from the operation, the flough became loofe; and the leaft motion of the tongue gave great pain. Upon examining the flough, there was a fmall fpot which looked red, and was furrounded by a dark furface; this was towards the right fide. Upon further examination it appeared, that the ligature to the right had not completely deadened the part at the centre, in which the artery had its courfe. This accounted for the red fpot, as well as for the pain the patient fuffered; and led me, on the feventh day, to difengage the ligature on the left, (which was almoft completely feparated,) by means of a pair of fciffars, and pafs another ligature through the grove to the oppofite fide, and tie it over the part not completely deadened. This gave great pain for a few hours, which was relieved by the ufe of tincture of opium.

opium. On the 8th day, the patient had leſs pain than on any preceding day, and leſs ſalivation; and, on the 9th, the whole ſlough came away. On the 13th, the tongue had ſo much recovered itſelf, that there did not appear any loſs of ſubſtance whatever, only a fiſſure of half an inch in depth, in the anterior part of it; and, as that now ſeemed to be exactly in the centre, there was not the ſmalleſt deformity.

General reſults of the preceding caſes or experiments.

The preceding caſes, in the view which it is intended to take in the preſent Paper, are to be conſidered as ſo many experiments, by which the ſtructure of the tongue is in ſome reſpects aſcertained: they enable us to draw the following concluſions.

Internal ſtructure of the tongue has little irritability.

The internal ſtructure of the tongue is leſs irritable than almoſt any other organized part of the body; therefore, the peculiar ſubſtance which is interpoſed between the faſciculi of its muſcular fibres, is not in any reſpect connected with the nerves which paſs through its ſubſtance to the organ of taſte, but is merely a ſoft medium, to admit of great facility of action in its different parts.

Its nerves eaſily deadened.

The nerves of the tongue appear to be more readily compreſſed, and deprived of their power of communicating ſenſation, than nerves in general; and any injury done to them is not productive of diſeaſed action in the trunk of the injured nerve.

Comparatively little inflammation and pain follow compreſſion of this organ.

If we compare the effects of compreſſion upon a portion of the tongue, with thoſe of a ſimilar compreſſion upon the hæmorrhoidal veins when they form piles, or thoſe of the teſticle in caſes of varicoſe veins of the ſpermatic chord, which not only produce very violent local inflammation, but alſo a conſiderable degree of ſymptomatic fever, it is impoſſible not to be ſurpriſed that the reſults ſhould be ſo very different; ſince we are led to believe, upon a general principle, that parts are ſenſible in proportion to their vaſcularity, and that all the organs of ſenſe, when inflamed, are more exquiſitely ſo than any other parts of the body.

It ſloughs off very early.

The tongue appears to have a power of throwing off its ſloughs in a ſhorter time than any other part. Eight or nine days is the ordinary time of a ſlough ſeparating from the common parts: in the boy's tongue, it was only five.

Having ſtated the information we derive from theſe caſes, reſpecting the ſtructure, ſenſibility, and irritability of the

 tongue,

tongue, it now remains to mention the advantage to be derived from them in a profeſſional view; and, although this is not directly in the line of the purſuits of this learned Society, yet, ſo ſtrongly is it connected with humanity, that it cannot be ſaid to be foreign to them, or undeſerving their attention.

Great benefit of this knowledge in caſes of cancer.

The information derived from theſe caſes, enables us to attempt with ſafety, the removal of any part of the tongue which may have taken on a diſpoſition to become cancerous. As this diſeaſe in the tongue always begins in a very ſmall portion of that organ, it is, in the early ſtage, more within the reach of removal than when in any other part of the body; and, as the glands of the tongue are independent of each other, the cancerous diſpoſition by which one of them is attacked, does not ſo readily communicate itſelf to the others; and the part may be removed, with a greater degree of ſecurity againſt a future recurrence of the diſeaſe, than in other caſes where this malady attacks a portion of a large gland, the whole of which may be under the influence of the poiſon, long before there is any appearance of its being diſeaſed.

## IX.

*Account of Three Mechanical Structures not commonly noticed or known, in which the Effect is produced by the Difference between the Actions of Two ſimple Inſtruments of the ſame Deſcription; namely,* 1. *a compound Barrel and Winch;* 2. *a compound Screw, and* 3. *a compound Pump.* W. N.

Properties of the wheel and axle.

IN the ſimple mechanical engine, called the wheel and axle, or barrel and winch, the advantage of the power over the weight is known to be as the radius of the wheel or winch is to that of the axle or barrel. If it were, therefore, propoſed to increaſe that advantage, the handle muſt either be made longer or the barrel more ſlender; neither of which can practically be done beyond certain limits in the uſual form; becauſe the handle might be too long for management, or the barrel too ſlight to ſupport its burthen. The celebrated George Eckhardt is, I believe, the inventor of the conſtruction, *Plate* III. *Fig.* 2. in which the part A of the barrel is larger than the part

A compound barrel or roller, by which the purchaſe may be greatly augmented.

part B, and the rope which paſſes under the pulley C and ſuſtains the weight D is wound upon each in contrary directions. Whenever, therefore, the handle is turned, ſo as to gather the rope upon the larger cylinder, it will be given off by the ſmaller: and, for every turn of the larger or its correſpondent portion of rope wound up, there will be given off a portion of rope, anſwering to the circumference of the ſmaller. Conſequently, the quantity of unwound rope will be leſs, after ſuch a turn, by a quantity equal to the difference between the circumferences of the two cylinders; and the weight D will be elevated through half that ſpace. Whence

As the circumference deſcribed by the winch (or its radius) Its power.

Is to half the difference between the circumferences of the cylinders (or half the difference of their radii)

So is the power

To the weight (in equilibrio.)

It is evident that the peculiarity of this engine is that the half difference of the radii may be made as ſmall as we pleaſe without weakening the cylinder or requiring any quick curvature of the rope; neither of which requiſites could be had in the ſimple cylinder. Its practical diſadvantage appears to be that a large quantity of rope will be uſed to produce a moderate elevation of the weight. Peculiarity of this engine.

The firſt account I find of the double ſcrew *Plate* IV. *Fig*. 1. is given by Mr. Wm. Hunter in the Philoſophical Tranſactions for 1781. In the ſketch before us, A B C repreſents the ſcrew of a preſs: the upper part of the ſcrew A has a coarſer thread than the lower B and a concave ſcrew or nut, C, receives the latter, while the former paſſes through a proper helical cavity in the frame of the preſs. In the preſent ſtate of the apparatus, the nut C is pinned faſt to its ſcrew by a croſs bolt; and conſequently, when the whole ſcrew is made to turn by the arms at the top, the nut C is carried round, and the effect is preciſely the ſame as if the preſs were governed by the ſcrew A alone. But when the nut C is diſengaged by drawing the croſs bolt, and likewiſe prevented from revolving by pinning it to the moveable platform, the effect becomes more compounded: that is to ſay, while the whole ſcrew is carried downwards by one revolution, through one thread of A, the nut C is drawn upwards through one thread of C. And Double ſcrew engine. One ſcrew preſſes down while another draws up, and the effect is produced by the difference of their actions.

 the

the confequence is the fame as if a fingle fcrew had been ufed, having the diftance between thread and thread equal to the difference beween the meafures of one thread in each.

Examples of flow actions with ftrong coarfe fcrews.

For example. Suppofe it were required to operate by ftrong preffure with a fcrew of ten threads in the inch, it is evident that the metallic protuberance or helix could not be quite fo thick as one twentieth of an inch, and could not therefore withftand any confiderable force. But by the prefent conftruction we may make the thread as ftrong as we pleafe. Thus if the flighteft be one quarter of an inch, the fineft fcrew muft have two turns in the inch, and the coarfeft two turns in one inch and one tenth; or in other words, it would pafs over the fame fpace in ten turns as the other fcrew would in eleven.

It is evident that by the conftruction in the figure, the moft powerful preffure need not be ufed till the fubject under compreffion has already been urged by the fimple fcrew.

Obfervation on new or unufual conftructions.

I may here remark concerning every one of the contrivances in this paper, that I have never confidered it as a requifite in offering a machine or inftrument to the attention of my readers that it fhould be univerfally better than the means already in ufe. If it be new or unheeded, and ingenious, or capable of adding to the ftock of refources which intelligent conftructors find fo various in their degrees of eligibility under different circumftances, it will, I am confident, be confidered with intereft, and accepted with welcome.

Confiderations refpecting the ufe of a double fcrew in meafuring.

When we confider the extreme precifion of a fimple fcrew, if commonly well made, it may feem fcarcely neceffary to ufe this contrivance in the meafurement of fmall quantities. The fineft fcrews ufually to be met with do not exceed 100 threads in the inch, and I do not know of any having been made finer than of 200 threads in that fpace. Let us confider by what threads of ufual or even coarfe number we could have a difference per turn of above one thoufandth of an inch, and we fhall find that the difference between a thread of 32 and one of 33 in the inch is the 1056 part of an inch. So likewife the difference between the 60th and 61ft of an inch is the 3660th pert, &c. I have a level trier on this conftruction which gives the four hundredth of an inch for each turn of its compound fcrew.

Pump with a double plunger by which the

*Fig*. II. *Plate* 4. is a pump defcribed to me by Mr. Bramah, and I think his own invention. Between the collar of leathers

A, there paffes a folid cylinder, of which the lower part B (diminifhed in diameter) paffes out through another collar of leathers D; the internal fpace or pump barrel fituate between thofe two collars being clofed on all fides, except where two pipes communicate, one of which, E, has a valve opening inwards, and the other D, has a valve opening outwards. Now when the folid cylinder or plunger is drawn up, the larger portion paffes for the moft part out of the cavity of the barrel; but inftead of being replaced by an equal mafs of water entering through the pipe E as ufual in the jack head pump, it is moftly replaced by the introduction of the folid part B, and the water which enters will be equal in its folid contents to the difference only between the folidities of the two cylindrical parts of the fame length. Or the effect will be the fame as if the barrel were of much fmaller dimenfions. In this way with the ftrength of conftruction and ample fittings of a large engine the power of a fmall one may be obtained, which in fome inftances of forcing may be defirable.

Space for water in a large bore is reduced to that of a much fmaller.

It fcarcely need be mentioned that the fame effect may be produced by two folid piftons working in diftinct barrels of different bore or ftroke, and communicating with each other.

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## X.

*Scoteography, or the Art of Writing in the Dark. In a Letter from Mr.* JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

PERHAPS you have remarked that my communications to your Journal are not all in the fame hand: the truth is, I am unable to write; and my ignorance of the art arifes from the lofs of fight in early infancy. The knowledge of the preceding circumftance cannot prove of the leaft moment to you; it is only mentioned by way of introduction to the prefent letter, which may afford an ufeful hint to perfons labouring under the fame difadvantage.

Hiftory of the invention.*

The perufal of Mr. Berard's method* of teaching the blind

* Phil. Jour. Nov. 1802, Vol. III. page 190.

to

to write was perhaps the firſt occurrence of my life which led me to think ſeriouſly of committing my thoughts to paper with my own hand.

Irard's method writing by the ouch only is not adapted to the early blind.

This gentleman's plan however preſented many difficulties which it is unneceſſary to enumerate; in reality it promiſes to be of much advantage to thoſe who have learned to manage a pen before they loſt the uſe of their eyes: but as this has not been my lot, I reſolved to conquer the hardſhips of fortune, if poſſible, by inventing an alphabet conſiſting of characters which a mechanical apparatus might form with eaſe and certainty. After ſketching an outline of a method in my own mind, I imparted my unfiniſhed project to an ingenious friend, being induced to aſk his aſſiſtance by a conſciouſneſs of my own inexperience in the art of writing. This gentleman completed the buſineſs in a ſhort time, by inventing a ſet of diſtinct characters of eaſy execution, and contriving the means of imprinting them upon paper.

Requiſites of the art.

The origin of the invention being now ſtated, the proceſs may be deſcribed in the following manner; including the preparation of the paper, the alphabet, the law of combining the letters, the apparatus, and the proper kind of ink.

Practice. The paper is iled into ſquare compartments.

The paper is prepared by dividing it into ſeveral ſquare compartments, which will appear in the annexed figure, page 57. The operation might be performed by means of a ruler, but the page may be ſtamped with greater certainty by the writing-frame; which inſtrument will be deſcribed in the ſequel.

The alphabet is ormed by placing a dot or a ſtroke in a different part of the ſquare for each letter.

The alphabet is formed by means of two marks only; theſe are a dot, and a ſtraight ſtroke; for each compartment may be conſidered as the ſkeleton of any character the writer wiſhes to make; and it is completed by placing the requiſite mark in its proper place. The letters might be deſcribed ſeparately, but a moment's inſpection of the alphabet given in *Fig.* 1ſt explains the ſtructure of it better than words can do. The ſcheme moreover ſhews, that there are thirteen places where the marks may be fixed, ſo as to form all the letters: to theſe may be added the three characters denoting the end of a word, a capital, and the numerals 1, 2, 3, &c.

Method of combining the letters or of writing.

Since placing a dot or a ſtroke in a ſquare makes a letter, the ſame compartment may form the baſis of ſeveral characters in certain circumſtances; provided the law of combining them be firſt eſtabliſhed.

My

My method consists in supposing the thirteen places of each compartment to be numbered, as is done in the annexed figure (*Fig.* 2d.) Now if, in spelling a word, the place of the former of two adjacent letters be of a less denomination than that of the latter, they may both be put in the same square; in like manner if the second do not exceed the third in value, this last may also be inserted with the other two; and the same rule will obtain with the fourth, &c. Letters of the same denomination, as o, r, &c. are to be placed in separate squares; and every capital must have a compartment to itself. This way of writing observes no proportion betwixt the length of a word and the space occupied by it: the monosyllable (and) fills three squares; which is the number taken up by the trisyllable (inspection) See *Fig.* 1st.

The elementary processes of the art being now explained, we come in the next place to the description of the instruments and their uses.

The writing-table is an oblong board, having two parallel brass plates passing from bottom to top, and containing a space between them, just sufficient to receive a sheet of paper: they are an inch in breadth, and ⅛ of an inch in thickness, being fixed with their flat sides to the table. A row of short pins is placed along the middle of either plate: each pin in one row is opposite to one in the other, and the distance of two adjacent pins in either plate, is exactly equal to the breadth of the writing frame. This instrument is an oblong piece of brass, a quarter of an inch thick: it has a bevil at each end, which parts pass over the side plates of the table, and thereby suffer the intermediate parts of the frame to rest upon the paper. The annexed figure (1st) shews the structure of the perforated part and the size of the squares.

The writing table.

The writing frame.

The process of stamping the paper consists in covering the underside of this plate with printer's ink; a small hole being made in either bevil for the reception of two opposite pins in the side plates; the operation commences by placing the frame on the highest pair, it is afterwards successively removed to each inferior pair until the whole page is stamped and prepared for the pen.

Operation of preparing the paper.

This instrument is a square brass rod, four inches long: each side of the square being nearly half as long, as a side

The pen.

of

of a compartment in the frame. One end of this pen is cut away parallel to one of its ſides, ſo as to form a plate three eighths of an inch long and a line in thickneſs: the end of the plate is alſo filed down ſo as to leave at one corner a point, which is parallel to the axis of the rod; this point is uſed in making dots. The other end of the rod is cut diagonally parallel to its axis, thereby forming a priſm, the end of which is an iſoceles triangle. The extremity of this priſm is alſo cut away, ſo as to leave a plate lying in the ſame plane with that deſcribed above: the end of this plane has a ſmall bevil externally, which gives it the edge of a chiſſel; and it is uſed in making ſtrokes. The writing frame always reſts on that part of the page, which you are going to write upon; and the ſides of the compartments together with their angles guide the pen to the proper places: the method of uſing which will eaſily occur to thoſe who are acquainted with the alphabet and apparatus.

Ink. They ſhould however be informed that *printer's ink*, is fitteſt for the purpoſe: it is ſpread upon a cuſhion of ſoft leather, which muſt be touched by the pen, as oft as it is applied to the paper.

JOHN GOUGH.

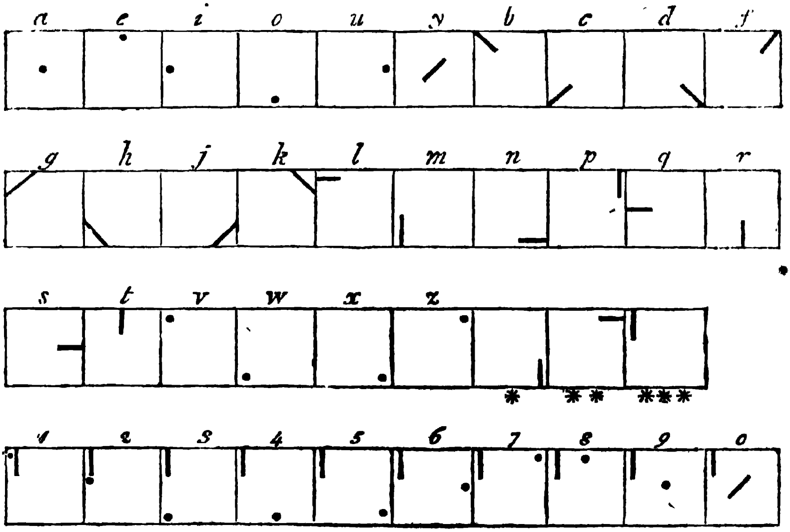
*Middleſhaw*, *Dec.* 15, 1803.

(*See alphabet, next page.*)

*P. S.* A paper of mine was read Nov. 4th before the Mancheſter Society, in which I endeavour to demonſtrate Mr. Dalton's doctrine of mixed gaſes to be repugnant to the mechanical philoſophy. It alſo proves experimentally that water augments the bulk of air of a given temperature without the intervention of a ſtopple of mercury; from which the nonexiſtence of an aqueous atmoſphere is inferred upon known principles.

ALPHABET.

ALPHABET. *Fig. 1ſt.*



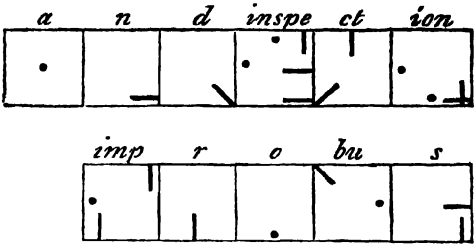
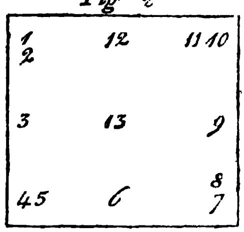
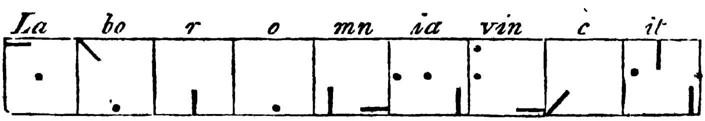




*Fig* 2




*Denominations of the Letters.*

| 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | 13 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| b | l | i | c | m | o | d | n | u | f | p | e | a |
| g | | q | h | | r | j | | s | k | | | y |
| v | | | w | | | x | | | z | | | |

* The ſquare diſtinguiſhed by one ſtar underneath contains the mark to denote the end of a word.

** The ſquare ſo diſtinguiſhed contains the mark denoting a capital letter.

*** The ſquare ſo diſtinguiſhed contains the mark ſignifying a numeral character or number.

XI. *Method*

## XI.

*Method of uniting Sulphur and Phosphorus without Danger to the Operator, and an Attempt to explain the Change that then takes Place. By* ROBERT BRIGGS, M. D. *Edinburgh. Communicated by the Author.*

Danger of combining fulphur and phofphorus.

MR. ACCUM has lately favoured your readers with an account of fome experiments made on the compound of fulphur and phofphorus, fhowing the great danger of preparing it by the ufual procefs, but without pointing out a method by which it may be readily formed, and that without the fmalleft rifk in the hands of a careful operator. I fay careful operator, for no preparation of fulphur and phofphorus fhould be handled carelefsly. In order to prevent effects fimilar to thofe which happened to Mr. Accum, is the purport of this paper.

Limited combination of fulphur with phofphorus under 180°.

If equal quantities of fulphur and phofphorus are melted together under water at a temperature not exceeding 180° of Fahrenheit's thermometer, a certain proportion only of the fulphur unites with the phofphorus. The remainder is eafily wafhed away with cold water. This does not hold in higher temperatures: any quantity of fulphur may then be united.

Qualities of this compound.

Upon examining the compound after it is perfectly cold, it will be found to have a friable texture, fpungy appearance, and a fulphur colour. The heat of the hand is fufficient to melt it, and a temperature of about 112° or nearly fo will fet it on fire if perfectly dry. To dry it perfectly, however, is very difficult. When juft formed, and during its formation, there is a ftrong fmell of fulphurated hydrogen, and a confiderable quantity of that gas makes its efcape. After the heat of the compound under water is raifed to 200° or 210°, the water is decompofed very rapidy; white fumes, which before filled the empty fpace of the flafk, now iffue out of its mouth in abundance, and if the veffel is fhaken by the hand or by allowing the water to boil, a fudden inflammation and explofion is the confequence. The compound of fulphur and phofphorus begins to decompofe the water immediately after the union of the two commences, but, towards the boiling point of water, that decompofition is very rapid.

If the heat be raifed nearly to boiling, the water is rapidly decompofed, white fumes efcape, and it explodes.

If

If you take the veſſel of the fire juſt when the white fumes have filled the flaſk, but, before they begin to iſſue from its mouth, and place it in a dark room without ſhaking it in the leaſt, the whole ſpace above the water which is filled with the denſe white fumes, has a beautiful appearance, like an aurora borealis. At this time very frequently, though not always, bubbles of phoſphorated hydrogen gas, or rather ſulphur phoſphorated hydrogen gas, if I may be allowed the term, eſcape and take fire on coming in contact with the external air, forming a beautiful white ring of ſmoke riſing to the cieling of the room,—juſt ſuch as happens from the decompoſition of water by means of phoſphuret of lime or potaſh.

The white fumes are beautifully luminous in the dark, and fiery rings ſometimes appear.

Now what is the change produced during this formation? That the water is decompoſed, is abundantly evident. But while the hydrogen of the water eſcapes, what becomes of its oxigen? If the oxigen were alſo allowed to eſcape, we ſhould feel its preſence, from exploſion after exploſion taking place, immediately after it left the water along with the hydrogen, united with as much phoſphorus as is ſufficient to fire the two.

In this proceſs, though the water is decompoſed, the oxygen is not ſet free.

The firing, however, only happens after the ſulphur phoſphorated hydrogen reaches the external air, or when the decompoſition goes on very rapidly from a high temperature, and faſter than the oxigen can be diſpoſed of. From all theſe facts taken together, I believe it muſt be pretty obvious, that the compound which we form of ſulphur and phoſphorus under water, is not of pure ſulphur and phoſphorus, but of their oxides. That is, during the junction of ſulphur and phoſphorus by heat under water, part of the water is decompoſed, the hydrogen eſcapes after engroſſing a ſmall portion of ſulphur and phoſphorus, while the oxigen unites with the ſulphur or phoſphorus, or both, converting them into an oxide of ſulphur and phoſphorus. This will be found farther confirmed from conſidering the nature of this compound. It is very inflammable, much more ſo than a compound of pure ſulphur and phoſphorus formed by another method which I ſhall deſcribe by and by; but which may be converted into an oxide by a very ſimple proceſs, which I ſhall alſo point out; then it becomes one of the moſt inflammable bodies with which I am acquainted. Farther the compound of oxide of ſulphur and phoſphorus, does not decompoſe cold water ſo far as I have been able to perceive, though the contrary is ſaid by Mr. Accum; but it will decompoſe the water if heated.

It is employed in oxiding the baſes.

which oxide is more inflammable than the ſimple compound.

Phoſphorus and ſulphur do not decompoſe water unleſs heated.

Upon

The compound first mentioned is of the oxide; and the phosphorated and sulphurated hidrogens contain oxigen.

Upon the whole then, I imagine, that, this compound when formed under water, is nothing else than a mixture of the oxide of sulphur and the oxide of phosphorus, and that phosphorated hydrogen gas, is not phosphorus dissolved in hydrogen gas, but a solution of the oxide of phosphorus in that fluid, and the same may be said of sulphurated hydrogen. It is not sulphur, but an oxide of sulphur that is dissolved.

Best method of forming it.

I shall now proceed to mention what I consider as by far the best method of forming a junction of sulphur and phosphorus, and of afterwards converting them into oxides without risk or danger.

When I first began to examine the nature of the oxides of sulphur and phosphorus, the decomposition of water, and the escape of its hydrogen only, at a low temperature, I imagined that I could effect the union of the two much better without the water.

The mixture heated in a phial covered with a perforated bladder.

I took a phial glass and filled one third of it with the usual proportions of sulphur and phosphorus, I tied a piece of strong bladder over the mouth of the glass, and made a small hole in it with a pin. I expected that after the phosphorus began to melt, probably it would fire with the small quantity of oxigen of the atmospheric air in the phial, before the sulphur and phosphorus had joined. I therefore used the precaution of a small hole in the bladder to allow the escape of the rarefied azotic gas. It did fire, and was followed by a trifling explosion.

The heat was by this suddenly augmented, the union of the sulphur and phosphorus formed immediately, and nothing more particular happened. The heat, however, is raised so suddenly that if the phial is large it is very apt to break. I would not therefore recommend this to be tried by a young chemist without great caution.

Explanation of Mr. Accum's accident.

Before going farther, I would now attempt to explain the accident that happened to Mr. Garden as related by Mr. Accum. The compound of oxide of sulphur and phosphorus which he was melting with the phosphorus, had either not been thoroughly freed from water, which it is very difficult to do, and hence the explosion is easily explained on the principles which I have already related; or, allowing that there was no water, the phosphorus had been fired by the oxigen of the atmospheric air in the upper part of the phial, at the very moment of shaking

ſhaking the glaſs. This of courſe would raiſe a quantity of the melted phoſphorus, and at the ſame time oblige him to remove his finger from the mouth of the phial. The highly rarified nitrogen would ruſh out with violence, carrying part of the melted phoſphorus along with it, an exploſion and its effects would be the conſequence.

A pretty loud report may be produced by heating any portion of air in a phial glaſs covered by the finger, on ſuddenly removing the finger.

This explanation, however, is rendered abundantly evident from what follows. Fill a phial glaſs, or what is ſtill better, a thin glaſs tube ſhut hermetically at one end with ſulphur and phoſphorus. Cork it firmly, and plunge the tube into hot water, the heat of which is to be gradually augmented till it boils. The ſulphur and phoſphorus will melt and unite peaceably. Shake them after taking the tube from the boiling water, that the mixture may be compleat, and no accident will happen.

Experiment in proof. Phoſphorus and ſulphur gradually heated, in a ſmall cloſed veſſel, unite without danger.

It is hardly neceſſary to caution againſt plunging the tube into boiling water at firſt. A contrary plan allows any ſmall quantity of oxigen that may be preſent in the very ſmall portion of air that may remain between the particles of the ſulphur and phoſphorus to be taken up by the phoſphorus before melting. If a compound of ſulphur and phoſphorus be prepared as above directed, having only a ſmall proportion of ſulphur, it has a yellowiſh white colour, is ſolid when cold, and has a cryſtallized appearance. This ſubſtance is more inflammable than phoſphorus, but not by any means ſo much ſo as a dry oxide of ſulphur and phoſphorus. This compound, however, may be eaſily converted into an oxide by the following ſimple proceſs. Set fire to the mixture, ſtill in the tube, by taking out the cork, admitting a little air, and plunging a hot wire into it; allow it to burn only five or ſix ſeconds. It will by this be converted into the oxide of ſulphur and phoſphorus, be found to reſemble in appearance very much a neutral ſalt efflorefcing, ſublime into the neck of the glaſs, and be ſo very inflammable that the inſtant you bring it into the air it catches fire, though the thermometer be at the freezing point.

Not ſo if the heat be ſudden.

This compound is leſs inflammable than the firſt mentioned oxide:

into which it may be coverted by combuſtion.

*Edinburgh, December* 15, 1803.

On

# XII.

## *On the beſt Method of aſcertaining the Dip at Sea. In a Letter from Mr.* EZEKIEL WALKER.

To Mr. NICHOLSON,

SIR,

The dip of the horizon rendered very uncertain by refraction.

IT is well known that refraction varies ſo much near the horizon as to render the depreſſion or dip very uncertain, whence it ſeems deſirable, in the preſent improved ſtate of navigation, to find the dip by obſervation rather than truſt to the table. An inſtrument ſo conſtructed as to meaſure an angle of 180° would be very convenient for this purpoſe, and the back obſervation in Hadley's quadrant has been propoſed; but the back obſervation is attended with ſo many inconveniences to the obſerver, and ſo little to be depended upon, as to render it nearly uſeleſs. The inſtrument which I have propoſed for taking any angle leſs than 180° ſeems well adapted to this purpoſe *. The diſtance of the two oppoſite horizons at ſea is = 180° + twice the dip; but the motion of the ſhip renders this method very troubleſome and uncertain; for whatever uncertainty attends taking an altitude of a celeſtial object, that uncertainty is doubled in taking the diſtance of the two oppoſite horizons. To avoid this inconvenience, let the meridian altitude of the ſun's upper or lower limb be taken with a ſextant by one obſerver, and at the ſame time, let another obſerver, with an inſtrument ſo conſtructed as to meaſure an angle of 180° take the diſtance of the ſame limb of the ſun, from the oppoſite horizon; the ſum of theſe two angles – 180° is = twice the dip. And as two obſervations of the ſun's altitude are thus taken independently of each other, the latitude will be obtained with greater preciſion than by a ſingle one.

The back obſervation will not correctly ſhew the dip.

It is beſt found by taking the ſun's altitude and its oppoſite ſupplement by two perſons;

The obſervations may alſo be taken with two inſtruments by one perſon.

or by one perſon with two inſtruments.

Let the obſerver begin ſome time before noon, and take the altitude of the ſun's limb. 2. With the other inſtrument take the diſtance of the ſame limb from the oppoſite point of the

* See Philoſophical Journal, vol. iv. p. 218. and vol. vi. p. 219.

horizon,

horizon, then take the firft inftrument, and correct the obfervation; and fo continue to ufe the inftruments alternately till the fun fhall arrive at his greateft altitude.

If the fun's altitude were taken, when off the meridian, by two obfervers, in the manner above mentioned, in order to find the hour from noon, it might greatly add to the accuracy of that important problem. This method is recommended in finding the time.

## REMARKS.

From the above ftrictures it appears, that navigation might receive many improvements from an inftrument that would take in the largeft angle, and admit of being accurately adjufted. The latitude and time of the day might then be determined with greater precifion than at prefent, and the limits of the method of finding the longitude by the lunar theory would be enlarged.

EZEKIEL WALKER.

*Lynn, December* 22, 1803.

*(To be continued in a future communication.)*

ERRATUM.

Vol. VI. p. 219. near the bottom, for 120°, read 180°.

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## XIII.

*New Procefs for fabricating Alum artificially and without the Affiftance of Evaporation. By Cit.* CURAUDAU, *Correfponding Member of the Society of Apothecaries of Paris* *.

A REPORT was made to the National Inftitute in Fructidor of the year 9, by Citizens Guyton aad Vauquelin, on a new procefs of Citizen Curaudau, for the artificial fabrication of alum. As this report is not generally known, we haften to communicate it to our readers.

Citizen Curaudau propofes, in the fabrication of alum, to employ one hundred parts of clay, and five of muriate of foda, diluted with a quantity of water fufficient to give a pafty confiftence to the mixture. It is then made into loaves, with which Process employed by Cit. Curaudau to make alum artificially. 100 clay and 5 common falt kneaded with water; the loaves

* From the Annales de Chimie, Floreal, An. XI. No. 137.

are ignited, cooled, pounded, and one fourth of fulphuric acid water is added by degrees till 10 times the acid.

which a reverberatory furnace is filled, and wherein a brifk fire is kept up for two hours, or until the infide of the furnace is obfcurely red. The calcination being finifhed, the clay is reduced to powder and put into a found cafk; one fourth of its weight of fulphuric acid is then poured over it, at feveral intervals, agitating it ftrongly upon each addition. As foon as the diffengaged vapours of muriatic acid are diffipated, a quantity of water equal to that of the acid is added, and it is ftirred as before. This produces a combination between the acid, the earth, and the water, fo rapid, that the mixture heats, fwells and emits abundance of vapours. Laftly, when the heat has abated a little, water is continually added until the quantity amounts to about eight or ten times that of the acid.

To the clear decanted liquor potafh is added.

When that part of the earth which is ufelefs in the formation of the alum is depofited, and the liquor become clear, it is drawn off into veffels or boilers of lead. A quantity of water equal to that of the liquor drawn off, is then poured over the refiduum, and afterwards added to the firft: laftly, a folution of potafh is mixed with the lye, in which the alcali amounts to one fourth of the weight of the acid employed, and it is agitated. If the preference be given to employ fulphate of potafh, the quantity ufed muft be double that of the alcali.

The alum cryftalizes by cooling.

After a certain time, the liquor by cooling, affords cryftals of alum, the quantity of which, when the cryftallization is completed, amounts to three times the weight of the acid made ufe of. This alum is refined by diffolving it in the fmalleft poffible quantity of boiling water; and it is then as pure as the beft alum of commerce.

The wafhings employed in a fubfequent operation.

As the refidue ftill retains fome faline parts, Citizen Curaudau recommends wafhing it a third time with a quantity of water fufficient to extract all the falt, and to ufe this inftead of fimple water in a fecond operation; by which means, there will not be any lofs.

The mother-waters ufeful in making pruffian blue.

Thus, without the affiftance of heat, the greateft part of the alum formed in the operation is obtained, which is highly beneficial. The author recommends the ufe of the mother-waters, which ftill contain alum with fulphate of iron very much oxided, in the fabrication of pruffian blue, to which purpofe they are well adapted.

He

He confiders the fabrication of alum as particularly advantageous to the manufacturers of pruffian blue, becaufe they may calcine their clay at the fame time with their animal matters, without any increafe of expence. They will have no need in that cafe to add potafh; and the prefence of iron, inftead of being injurious, will, on the contrary, be very ufeful. If they are defirous of fabricating alum for fale, they may ufe the folution of fulphate of potafh arifing from the wafhing of their pruffian blue, and which is ufually loft, inftead of water, to diffolve the combination of alumine and fulphuric acid. The refiduums or refufe of the diftillers of aquafortis are equally applicable to the fame purpofes: they contain the alumine and potafh requifite to the compofition of alum, and it will be fufficient to fprinkle this fubftance, reduced to powder, with fulphuric acid, and to add the neceffary quantity of water to the mixture, proceeding as has been directed above. The mother-waters of thefe alums are alfo ufeful in the fabrication of pruffian blue.

This procefs very beneficial to the makers of pruffian blue.

Refufe of the diftillation of aquafortis may be ufed.

We may alfo obferve on this occafion that the refufe of the diftillers of aquafortis contains more alcali than is required for the faturation of the fulphate of alumine formed by the clay, and that to obtain the greateft poffible quantity of this fubftance, it is neceffary to add an eighth by weight of this earth calcined, according to the procefs of Citizen Curaudau; and that in ufing about fixty parts of acid to a hundred of this earth, at leaft one hundred and eighty parts of very fine alum are obtained.

It contains an excefs of alcali.

Citizen Curaudau affirms, that he has for a long time prepared alum by thefe different proceffes with an advantage of more than 25 per. cent. over that of commerce, and that, notwithftanding the price to which it has now fallen, he ftill derives from 10 to 12 per. cent. profit. Laftly, the manufacturers of pruffian blue, to whom the potafh would be no expenfe, would at this time gain from 17 to 18 per. cent. by making this falt artificially.

Profits from thefe proceffes (in France.)

From the contents of this report, is is feen that the memoir of Citizen Curaudau contains the refults of experiments made on a large fcale, highly interefting to manufacturers and merchants, and that, in this view, it will be ufeful to circulate them through the channel of the fcientific Journals. We are therefore of opinion, that the clafs will give much credit to

Citizen Curaudau for having publifhed the refult of his obfervations, and that it ought to encourage him thus to turn the product of his ufeful labours to the advantage of his country.

Done in the clafs of phyfical and mathematical fciences, the 11th Fructidor, in the year IX.

Signed, GUYTON and VAUQUELIN.

The clafs approves the report and adopts the conclufions.

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## XIV.

### *Letter from Mr.* JOHN FARCY, *concerning the great Fiery Meteor of Nov. 13, and on other Subjects.*

To Mr. NICHOLSON.

SIR,

Correct date or the appearance of the fiery meteor in November laft. *It was on the 13th.*

THE great value of your Journal, as a record of facts connected with fcience, induces me to requeft you will correct the errors which have unfortunately crept into your account of the meteor of the 13th ult. which is defcribed, in the fecond line of the title, and in the firft line of the account at page 279, to have happened on the *fixth;* and alfo in the title of the cuts in the next page, where it is defcribed as an event of the *fixteenth.* I am much concerned that your friend who faw this interefting phenomenon over St. Ann's church-yard, did not give an eftimate of its elevation, or of its neareft approach to the zenith, or of the number of feconds during which the light occafioned by it continued.

Inquiries about its courfe, &c.

I was not fo fortunate as to fee it myfelf, or fince to meet with any fcientific perfon who did, but on the following evening, I enquired minutely of four or five watchmen of St. Martin's and St. Margaret's parifhes, who were in different places at the time in their way to their refpective watch-houfes; I went with each of thefe perfons to the fpot, and heard their relation of every circumftance which they faw; one of thefe perfons being juft by the Horfe-Guards within the Park, and another in the front of the Admiralty, agree in fixing the time at about twenty-nine minutes paft eight by the Horfe-Guards clock; from their comparifon of its height with the feven ftars then vifible (though it was rather hazy) I am inclined to think that

that it paſſed within about 30° of the zenith, to the South of it, and in a direction from E S E. to W N W. by cauſing theſe perſons ſeparately to count one, two, three, &c. as long as they thought the light laſted, while I looked at my watch, I conclude its light laſted but five ſeconds, which agrees very well with the two or three ſeconds in which it was ſtrong enough to affect perſons in a room, as mentioned by you at page 279.

Greateſt altitude about 60°, and courſe W.S.W.

During the very heavy fall of ſnow on Monday evening the 5th Inſt. as I was returning home from Drury-Lane Theatre, I ſaw at diſtant intervals, four or five ſudden flaſhes of light, which attracted the notice of many perſons near me, and was concluded to be lightning: has lightning been obſerved during the formation of ſnow in the atmoſphere? The air felt rather warm at the time.

Flaſhes of light during a fall of ſnow.

Your candour will, I truſt, excuſe me in lamenting the too conciſe deſcriptions given, at pages 218 and 219, of the ſyphon, and of the ſubſtitute for a fly wheel; it has not appeared evident to me that the ſyphon would act at all, without a ſtop cock below the funnel, *Fig.* 3. In line 7 of page 219 the printer has evidently made ſome miſtake, and what is given below in explanation of *Fig.* 2, is ſo far from evident, that I hope you will at your leiſure give a more detailed account. In page 220, laſt line but one, it ſhould refer to *Fig.* 4 inſtead of 3.

Obſervations upon ſome articles in this Journal.

You will greatly oblige me by giving a place in your Journal to a paper of Mr. Playfair's in the Edin. Trans. vol. V. 1802, containing Theorems on the Figure of the Earth. And to a paper in the Trans. of the Linnean Society, deſcribing an extraordinary appearance of the layers of flint in the chalk ſtrata of the Iſle of Wight. I am in haſte,

Valuable works indicated.

Sir,

Your obedient humble Servant,

JOHN FARCY.

*No.* 12, *Crown-Street, Weſtminſter,*
*Dec.* 15, 1803.

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REPLY. *W. N.*

Having received Mr. Woulfe's communication late in the month, I gave the account nearly in the conciſe words of the inventor.

inventor. I will take an early opportunity of ſpeaking more fully reſpecting it. In Mr. Howard's ſyphon the funnel part may in ſome meaſure be conſidered as a ſubſtitute for the ſide ſuction pipe. In large ſyphons neither will operate unleſs there be a ſtop cock at the bottom of the outer leg: in ſmall inſtruments, ſuch as that in the drawing, the ſtoppage is made by the finger.

The principal intention of a periodical journal being to publiſh in the form of a book ſuch eſſays as from their conciſeneſs might neither be circulated nor preſerved adequately to their merits, it ſeems to follow that larger works muſt for the moſt part be exceptionable from the ſpace they occupy. The excellent treatiſe of Profeſſor Playfair being in Engliſh, and of conſiderable length, has not yet been reprinted in our work, chiefly for the latter reaſon; but I will avail myſelf of the obliging indications of Mr. Farcy to re-conſider both the works he mentions.

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## XV.

### *Method of reſtoring rancid Eſſential Oils.* By J. B. De Roover.*

Proceſs for purifying rancid volatile oils.

DURING my long and frequent experiments on eſſential oils, I have diſcovered a method of reſtoring them, when rancid, which appears to poſſeſs great advantages. The experiment which led me to it is as follows:—

One ſixteenth of ether added to the oil, and left in digeſtion; five pints of alcohol added. Diſtillation on the water bath. Moſt of the oil remained behind.

I mixed one ounce of ſulphuric ether with ſixteen ounces of oil of peppermint ſlightly rancid; I left this compound in digeſtion for twenty-four hours, and then added five pints of ſpirits of the ſame mint. The ſolution was complete. I filtered the liquor and diſtilled it over the water bath. I expected that nearly the whole of the oil would have paſſed over in the diſtillation along with the alcohol and the ether; but, on opening the ſtill, I was much ſurprized to find the greater part of the oil ſwimming on the ſurface of the reſidual fluid. To aſcertain the quantity of oil which had paſſed over, I precipitated

* From the Journal of Van Mons (to whom it was communicated by the author,) V. 107. No. 13.

tated it from its ſolution in alcohol by water, according to my method, and four ounces and a half of oil were ſeparated as tranſparent and colourleſs as water.

Oil was ſeparated from the alcohol by adding water.

I then poured the reſiduum of the ſtill, together with the ſpirit and the water of precipitation, after having ſeparated the oil, into an apparatus over a naked fire, and diſtilled a ſecond time. I had the ſatisfaction to find a ſpirit riſe, the water of which, that came over at the ſame time, depoſited an oil as perfect as before. I repeated this cohobation by pouring the mixture of ſpirits and water on the remaining oil through the neck of the alembic until I had collected in all twelve ounces and a half of oil as fragrant and free from rancidity as the freſheſt and beſt oil of peppermint.

Second diſtillation of all the products.

Cohobation and re-diſtillations, gave a pure oil.

I rectified the ſpirits and judged by ſome trials that it might contain rather more than one ounce and a half of oil.

Some oil in the alcohol.

What remained in the ſtill was a thick adheſive matter of the nature of turpentine. I boiled it with water, and obtained ten gros (drams) of a ſort of dry turpentine ſimilar to reſin.

Reſinous matter in the ſtill.

It is difficult to aſcertain in what manner the addition of alcohol, or ether, contributes to the reſtoration, and in ſome meaſure the regeneration of ſo large a quantity of eſſential oil, which had loſt the greateſt part of its peculiar odour.

Theoretical ſpeculation.

Formerly it would have been ſaid, that the ſpiritus rector, with which the alcohol of peppermint was impregnated, had produced this effect; but now when we know that the ſmell of an eſſential oil does not depend on a peculiar principle, but on a conſtitution proper to the oily compound, we cannot admit that the oil of the alcohol had rendered any particle of the diſaromatized oil odorant; but we muſt ſuppoſe that the ether or the alcohol are in their decompoſition identified with the ſubſtance of the oil.

In fact, the alcohol, and more eſpecially the ether, contains as a conſtituent part hidrogen, which is the baſe of almoſt all the odours. Alcohol, ether, and the eſſential oils, are equally diſpoſed to undergo an alteration in the relative proportion of their principles. There is conſequently no obſtacle to our admitting this concurrence of chemical operation for the reſtoration which I have obtained of the oil of *mint*.

Another circumſtance tends to ſupport this explanation. I at firſt attributed the oil which I obtained in the ſecond operation to the difference of temperature of the bath, and of the

naked

Theoretical fpeculation on the purifying of volatile oils.

naked fire. But the oily products of the fubfequent diftillations being all made at the fame temperature, left me no caufe for hefitation in explaining the effect by a fucceffive re-action of the ether or the alcohol on the hidrogenated oil.

A third circumftance in confirmation of this opinion is, that the quantity of alcohol appeared to me to be fenfibly diminifhed, and that in the fmell of the oil, which was perfectly pure, I did not find the leaft trace of that of ether, which is fo eafily diftinguifhable. It is true that during the repeated diftillations, the ether might have been diffipated as well as the portion of alcohol, which conftituted the difference between the original and fubfequent quantities of that liquid.

It remains, neverthelefs, to be explained how a principle of the alcohol, by affimilation with the principles of the oil, fhould produce precifely the proportional compound which forms the fpecies of oil on which it re-acts. To this it may be anfwered that the bafe of this oil is only difpofed to take that principle which fhall replace it in its original ftate.

But even fuppofing this explanation to be unfounded, and that the influence of the ether and the alcohol fhall appear to be merely concurrent circumftances for producing a feparation of the particles of oil and reftoring its qualities, it is neverthelefs certain, that the fame effect has not hitherto been produced by any other means, and that my experiment furnifhes us with a valuable method of reftoring to commerce an immenfe quantity of effential oil, which would otherwife be of no value.

Second experiment.

*P. S.* Since the above mentioned experiment, I have made another trial with an oil which flowed as thick as turpentine, and had no fmell by which the plant from which it was extracted could be diftinguifhed. I mixed four ounces of this oil with two drams of fulphuric ether, and after four days diftilled it with pure water. Two ounces and a half of oil came over as clear and as fluid as fpirits of wine, having the beft and moft decided odour of wild thyme. There remained in the ftill two ounces of refinous matter of a dark red colour.

In this experiment the ether certainly developed the fmell of the oil on which it re-acted, or rather it recompofed the oil by tranfmitting its hidrogenous principle to the oily bafe.

Queftions

## XVI.

*Questions, 1. respecting the Place of the erect Image behind a Concave Speculum; 2. and of the Image formed by a Concavo-convex Mirror which is not Left-handed, and has the Property of revolving on its Axis along with the Mirror, but twice as fast; and 3. the Figure of the Sky. By a Correspondent.*

To Mr. NICHOLSON,

SIR,

I HAVE a general knowledge of the delightful science of optics, but have no pretensions to an intimate acquaintance with its depths. I have therefore presumed to request you will either give your own sentiments on the following difficulties, or lay them before your readers, some of whom I hope will give a popular explanation of them. Questions proposed.

1. We are taught by all the writers on vision that the distance of any object is ascertained from the divergence of the rays from any assumed point on its surface, and that as the sense or judgment refers to the point of divergence, we may have a perception of an optical image by reference to that region or part of space from which the rays in their last course are directed, whether they have in absolute fact proceeded from this last point or not. Thus it is that we see an image behind a looking glass though the rays have never actually proceeded from that image; but only move in the same directions as if they had done so. If the mirror be convex, the image will be nearer to its surface, because the rays will be more scattered and divergent, and seem to have come from a nearer point. And parallel rays are said to come from an object infinitely distant. General statement of the means by which we infer the positions and distances of visible objects. Looking glass.

Now Sir, my query respecting the concave mirror is this:— When I hold my face close to the surface, the image and object touch each other. If I recede, the erect image recedes as the rays become less and less divergent. Every one knows that when I am at the distance of half the radius, the rays are returned parallel, and consequently the image is then infinitely distant. When I move farther off, the rays become convergent, and therefore in a certain sense may denote an image farther off than at an infinite distance; but I have no wish to discuss this point. When I am at the center of the sphere all is Phenomena of the images in a concave mirror.

confusion, and beyond that point I begin to discern the inverted image that hangs in the air, and is formed by the divergence of the rays from their focal intersections.

Apparent recess and approach of the erect image in a concave mirror.

Such are the facts, but not the whole of the facts. For I am very sure, that after the first receding of the erect image, it comes to a place where it is stationary, and then instead of remaining at any situation that may be called an infinite or more than infinite distance, *it comes forward again* and continues to advance till it is lost in confusion.

Difficulty stated. Why does the image advance?

Perhaps I may not have expressed myself with all the clearness I could wish; but I think I have shewn a difficulty and the reason why it puzzles me. The true science of nature must be adequate to the explanation of all phenomena, or at least it must not be contrary to any one of them. What is the solution of this?

Optical mirror at Merlin's.

2. There is an optical mirror at Merlin's Exhibition in Hanover-street, in which you see your face perpetually changing from one distortion to another, arising from its greatest length varying through all its several diameters. So that the face becomes absurdly long, crooked, broad, &c. If you move farther off, the face has its natural aspect, but is seen turning round its center and successively assuming the erect and inverted positions, and every intermediate degree of obliquity.

Singular distortions, and revolution of the image.

A polished button of the same figure.

While these phenomena, produced by an apparatus concealed in a box, employed my occasional reflections, I happened to see a polished metallic button in the shops, which upon examination proves to be a mirror of the same kind as the larger one used at Merlin's. If it be considered in the direction of one of its diameters, it is a concave speculum; but in the direction of another diameter at right angles to the former, it is convex: and the radius of each curvature is about one inch. It is the same figure as a turner might produce in the lathe by using a chizel with a circular edge of one inch radius to scoop out a portion of a revolving piece till its smallest diameter, or calliper, became equal to two inches. Its effects are,

Particular description of the images produced by the button.

When the eye is brought very near the polished surface, it appears elongated in the direction of the diameter or chord, or of the concavity, and shortened in the diameter at right angles to that diameter, and if the speculum be turned on its axis, the image is affected with the strange undulation before mentioned, and undergoes all the variations twice in one revolution

Undulation.

volution of the mirror. But when the eye is at a confiderable diftance, a fmall perfect image is feen, which when the diameter of concavity is horizontal, is erect but not left handed; that is to fay, if you move your right hand, the image alfo moves its right hand, inftead of the left, as happens in a convex and a plain mirror; when the diameter of concavity is vertical, the image is inverted, but ftill right handed; and in the other pofitions of the mirror it takes every poffible fituation of obliquity, making two complete revolutions for one of the mirror itfelf. The image is larger the nearer the object, and the erect image is a little broader than the inverted; this deformity being more confiderable the larger the image. Small revolving image, not left-handed.

Now Sir, my fimple queftion is, *Where is this image?* Is it before the mirror, according to the property of the convex, or behind it, according to that of the concave. I thought to have difcovered this by the folar focus, or by applying a magnifier to the fmall image, but neither method anfwered my purpofe, and nothing within the fmall extent of my knowledge has enabled me to folve it by reafoning. Where is this image? before or behind the mirror.

3. The enlarged appearance of the moon near the horizon, and the flat vault prefented by the concavity of the heavens, have been explained by various allufions to the diminifhed light after paffing through a portion of the atmofphere, and its direction over a long row of objects, fuch as trees, houfes, &c. Something feems to be wanting in thefe accounts. Neither the fun or moon are ever fuppofed to be more diftant or larger when feen at confiderable elevations through clouds or mifts; and the fky feems flat, and thefe luminaries enlarged, whether the profpect be over woods, lawns, naked plains, or the calm fea. Befides which, I have no hefitation in infifting that the fky appears loftier on fome days than on others, and even at the fame inftant of time on different fides. If this be no error, what are the caufes of the appearance or the conviction produced in our minds? Flatnefs of the fky and horizontal moon. Not the fame at all times, &c.

I am, Sir,

Your obliged reader from the firft,

R. B.

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I believe my correfpondent would have vanquifhed his difficulties, with a little more confideration. If the explanation be not given by fome of my correfpondents, I fhall endeavour to give the replies he requefts in our next. W. N.

A New

## XVII.

### *A New Method of preparing the pure Gallic Acid. By* M. L. Schnaubert *.

Preparation of gallic acid.

FOUR ounces of finely powdered galls are infufed in water flightly alkalized with potafh; the infufion which is of a very deep colour is filtered, and a folution of nitrate of tin is dropped into it, until no more precipitate is formed. The free acid of the liquid is then faturated with potafh, taking care not to add the alkali in excefs, and the precipitate is feparated by filtration. The filtered liquid is then precipitated by acetite of lead, the weight and concentration of which is known. A greyifh white precipitate is thus obtained, which is to be digefted with diluted fulphuric acid, the proportion of the acid contained in which, muft be to the concrete acetite of lead as one to four. After a fufficient digeftion, the liquid is carefully filtered and evaporated, in order to afford cryftals. If the gallic acid thus obtained, fhould contain a fmall quantity of fulphuric acid, it may be deprived of it by digeftion, with gallate of lead.

In order to be certain of the quantity of fulphuric acid neceffary for the decompofition of a given quantity of gallate of lead, I previoufly diffolved four ounces of acetite of lead in fixteen ounces of water, and diluted one ounce of fulphuric acid in four ounces of water. By mixing thefe two liquids to afford the perfect precipitation, the requifite proportion was eafily found. I muft here obferve, that it will be better to keep the gallic acid in a liquid ftate than to reduce it to cryftals, becaufe evaporation always renders it more or lefs brown, I am firmly perfuaded that the gallic acid poffeffes the property of becoming brown by the action of light, at leaft, if in contact with the atmofphere. For if gallic acid be fublimed perfectly white, and then diffolved in water, it will be found after fome time to have undergone this kind of alteration. M. Bucholt has made a fimilar remark relative to the fame fact.

* Tromfdorff's Journal der Pharmacie, vol. ii. p. 61.

SCIENTIFIC

## SCIENTIFIC NEWS.

### *Reward of Twenty Pounds for the Artificial Production of Palladium.*

THE following is a copy of a paper received by me under cover, by the two-penny poſt. It is written in the ſame hand as a note which covered a ſmall piece of the palladium mentioned to have been received by me laſt Midſummer. (See Philoſ. Journal, June, 1803. Vol. v. p. 136.) Upon inquiry, I find, that Mrs. Forſter has received the ſum of 20£. with inſtructions conformable to this paper. This original is cut indent-wiſe on the margin, and has part of a manuſcript flouriſh or paraph on each border, but no ſignature.

(COPY.)

*December 16,* 1803.

SIR,

AS I ſee it ſaid in one of your Journals, that the new metal I have called palladium, is not a new noble metal, as I have ſaid it is, but an impoſition and a compound of platina and quickſilver, I hope you will do me juſtice in your next, and tell your readers I promiſe a reward of 20£. now in Mrs. Forſter's hands, to any one that will make only 20 grains of real palladium, before any three gentlemen chymiſt's you pleaſe to name, yourſelf one if you like.

**It is inſiſted that palladium cannot be formed by art; and a reward is offered for any proceſs to that effect.**

That he may have plenty of his ingredients, let him uſe 20 times as much quickſilver, 20 times as much platina, and in ſhort of any thing elſe he pleaſes to uſe: neither he nor I can make a ſingle grain.

Pray be careful in trying what it is he makes, for the miſtake muſt happen by not trying it rightly.

My reaſon for not ſaying where it was found, was, that I might make ſome advantage of it, as I have a right to do.

If you think fit to publiſh this, I beg you to give the names of the umpires, as I have ~~deſired~~ Mrs. Forſter to keep the money till next Midſummer, and to deliver it only in caſe they can aſſure her that the real metal is made by a certificate ſigned by you, and by them, on this check.

I hope a little bit of whatever is made may be left with Mrs. Forſter.

*Letter*

### *Letter from Lalande on the Meaſure of a Degree of the Earth in Lapland* *.

The meaſure of a degree in Lapland by Maupertuis, &c. ſuſpected of error.

ASTRONOMERS have long ſuſpected, that there was ſome error in the meaſure of a degree of the earth taken in 1736 in Lapland, by Maupertuis, Lemonnier, Outhier, and Celſius, becauſe this meaſure was greater than it ought to have proved according to all others. Mr. Melanderhielm has had the good fortune to obtain a repetition of this meaſurement. He informs me, that Mr. Swanberg, and three other Swediſh aſtronomers, have found the degree to meaſure 57209 toiſes in the latitude of 66° 20′, which are 196 toiſes leſs than by the French meaſurement, and gives for the flattening of the earth $\frac{1}{313}$. This agrees better with the other compariſons, and teaches us, that the figure of the earth is not ſo irregular as was ſuppoſed, from the degree meaſured in the north.

Re-meaſured lately by Swanberg, &c. who found it to be 57209 toiſes, or 196 leſs.

The flattening of the earth 1-313th.

Mechain gone to meaſure a triangle terminating at the balearic iſles.

Mr. Mechain ſet off on the 26th of April for Spain, whither he is gone to meaſure a triangle of 93000 toiſes, terminating at the Balearic iſlands, which will complete the grand and important meaſurement of the meridian, executed a few years ago by Meſſrs. Mechain and Delambre. He is accompanied by Meſſrs. Le Chevalier and Dezauche, and his youngeſt ſon. This meaſurement is very difficult, but no perſon is more capable of ſurmounting difficulties than Mr. Mechain, and we ſhall have the 54th degree in the middle of the whole arc meaſured by Frenchmen.

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New metal.

DESCOTILS has diſcovered a new metal in the ore of platina, it is preſumed that palladium may be an alloy of this new metal with mercury.

A blue equal to ultramarine.

Thenard has diſcovered a blue equally beautiful, and as fine as that of lapis lazuli, or ultramarine.

Prize of galvaniſm deferred.

The National Inſtitute has declared, that the galvanic prize will not be given this year.

*Theſe three articles are from the Journal of Van-Mons.*

* Journal de Phyſique, May, 1803, p. 400.

Letter

*Letter from Professor* PROUST *to J. C. Delumétherie on a dangerous fulminating Powder* *.

IN one of my late lectures, I made an experiment that might have been attended with serious consequences to some of my auditors.

Dangerous experiment.

It is, perhaps, no exaggeration to say, that the mixture of oxigenated muriate with arsenic, takes fire with the rapidity of lightning.

I am accustomed to compare the quickness with which different kinds of powder burn, by setting fire to them in cannons of equal diameter, passing each through a piece of cork placed in water, so as to sink in it almost their whole length.

Mode of comparing explosions with that of gun-powder.

In this manner I was desirous of burning the arsenical mixture, by the side of an equal quantity of gun-powder, in order to compare their flame and continuance; but I had scarcely time to draw back a little after setting fire to the two cannons, when the first exploded and burst, breaking the glass vessel, and throwing about the water in all the radii of the surrounding sphere. Though the mouth of the tube and the escape of the flame were perfectly free, the cannon was burst from top to bottom, and spread out as flat as a card.

A mixture of oxigenated muriate and arsenic burnt with the rapidity of lightning, burst the cannon that contained it, rendered it quite flat, and threw about the water in which it floated in all directions.

This powder is so violent in its effects, that I conceive it would be very dangerous, to attempt to make any use of it.

Its effects are too dangerous for use.

If two long trains be made on a table, one of gun-powder the other of this mixture, and they be in contact with each other at one end, so as to be fired at the same time, you will see with great surprize, that one disappears like a flash of lightning, while the other seems to burn with extreme slowness.

A long train of it disappears like lightning.

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*Letter from a Correspondent on the Expediency of converting Foreign Weights and Measures into English.*

To Mr. NICHOLSON.

SIR,

THE pleasure which your readers receive from the perusal of your Journal is so much abated by the obscurity arising from the names of the French weights and measures, that I

* Journal de Physique, May, 1803. p. 394.

wish

wish to suggest to your consideration, that a reduction of them to English terms, would be to you comparatively a small labour, in proportion to what it would be to the generality of your readers individually.

Your labours tend to the useful purpose of communicating science and general knowledge to the public of this country. To facilitate the acquirement of such knowledge, it should be communicated in the terms most easily intelligible to the people; not in such terms as require algebra, or a process in the Rule of Three, to give a precise idea of every measurement. As your Journal becomes more extensively circulated, there arises a stronger claim upon you from the public, in favour of the many who are too indolent, too busy, or too ignorant to expound for themselves this species of hieroglyphics.

I am, Sir,

Your constant READER.

---

ANSWER.

The rule proposed to be followed in all our translations, though perhaps not precisely, and on every occasion, adhered to, is, that wherever the foreign denominations of weight or measure are mere indications of the proportional quantities, (and may be considered as pounds, ounces, or grains, or as yards, feet, or inches all through,) the original names are used without embarrasing the subject with any reduction into fractional quantities: In every other case the reduction is, or ought to be made.

While I express my acknowledgments to this reader for his advice and remarks, I beg leave to say, that I heartily subscribe to his opinions as to facilitating the communication of knowledge, by the simplest methods. It is my wish in the side notes, and by every other means, to shorten the time, and assist the inquiries, of those whose other duties forbid the indulgence of deep or continued researches.

W. N.

*ACCOUNT*

## ACCOUNT OF NEW BOOKS.

*A Syſtem of Theoretical and Practical Chemiſtry, in Two Volumes with Plates. By* FREDERICK ACCUM, *Teacher of Practical Chemiſtry, Pharmacy and Mineralogy, and Chemical Operator to the Royal Inſtitution of Great-Britain. Octavo, Two Volumes, 733. and full Index and Contents, with Five Copper Plates.* Accum's ſyſtem of chemiſtry.

THIS work which, is dedicated by permiſſion to the managers of the Royal Inſtitution, and patronized by a very reſpectable liſt of ſubſcribers, is drawn up for the uſe of ſuch as are unacquainted with chemical ſcience. The author, therefore, begins by deſcribing ſuch experiments as are beſt adapted to exhibit the general nature of chemical action. The laws of affinity, and alſo thoſe which govern the phenomena of heat and light, are ſtated by the method of ſyntheſis, and illuſtrated by experiment. And theſe are ſucceeded by the claſſification of bodies; namely, firſt the principles undecompoſed, then the characteriſtic properties of the gaſes, and theſe are ſucceeded by the metals, alcalis, earths, &c. The diſplay of theſe, as to their properties and habitudes, by an orderly combination of general narrative and reaſoning, with particular facts, experiments or, operative proceſſes, conſtitute the buſineſs of the preſent work, which will doubtleſs prove acceptable to the numerous cultivators of the ſcience of chemiſtry.

*A practical Eſſay on the Analyſis of Minerals, exemplifying the beſt Methods of analyſing Ores, Earths, Stones, Inflammable Foſſils, and Mineral Subſtances in general. By* FREDERICK ACCUM, *Teacher of Practical Chemiſtry, Pharmacy, and Mineralogy. London,* 12*mo.* 183 pages. 1804. Accum's analyſis of minerals.

THIS active and indefatigable lecturer, has been requeſted by his pupils and auditors, to draw up a ſet of conciſe directions, to enable any perſon not intimately acquainted with analytical chemiſtry, to examine ſuch unknown minerals as he may meet with, and readily aſcertain their compoſition.

In

In the accomplifhment of this object he has exceeded his firft intention, and has not only given clear, and rather comprehenfive inftructions for analyfing all the different genera, and principal fpecies of the bodies mentioned in the title, and commonly met with, but has alfo given examples of analyfis of the rarer or lefs abundant mineral bodies.

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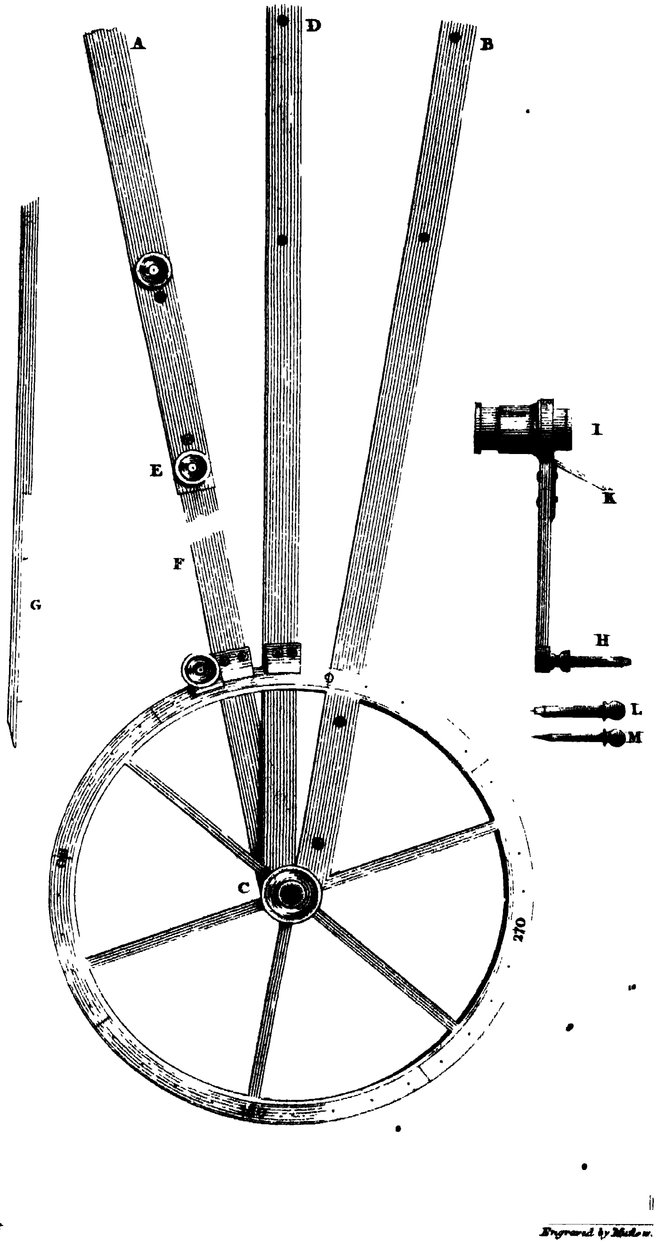
Boffut's hiftory of mathematics.

*A General Hiftory of Mathematics, from the earlieft Times, to the Middle of the Eighteenth Century. Tranflated from the French of John Boffut, Member of the French National Inftitute of Arts and Sciences, and of the Academies of Bologna, Peterfburg, Turin, &c. To which is affixed, a Chronological Table of the moft eminent Mathematicians.* 8vo. 566. p. *London,* 1803. *One Vol. Octavo.*

THIS juftly efteemed work is well tranflated under the infpection of Mr. Bonnycaftle, of the Royal Military Academy at Woolwich, who has added an introductory Preface, and the Table mentioned in the Title Page.

Station Pointer by Joseph Huddart Esq.r F.R.S.

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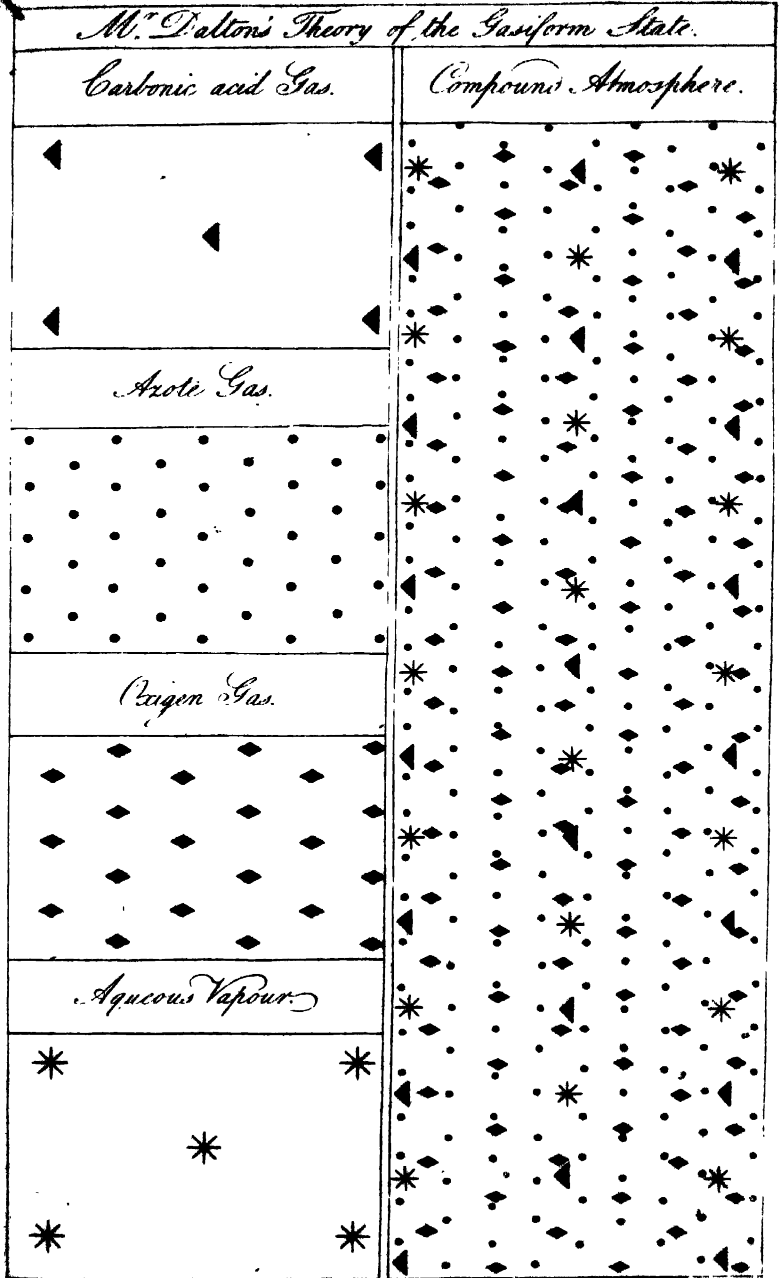


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Determination of Positions, by observation of three known objects

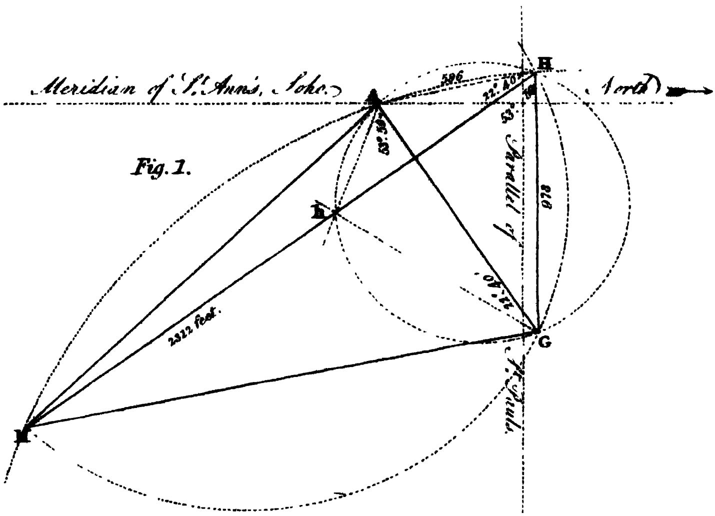
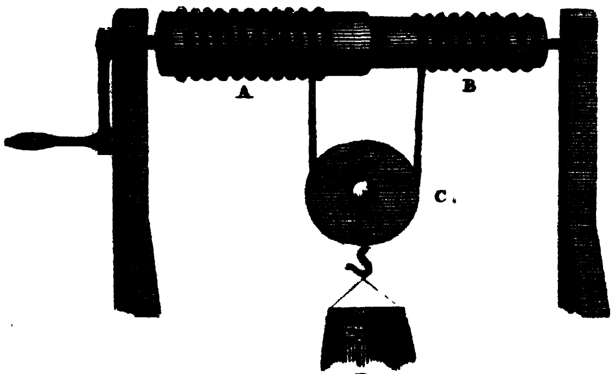



Fig. 1.

Winch & Barrell, (double)

Fig. 2.




Drawn by Blunt. Engraved by Mellow.

## Increase of power by a Double Screw.

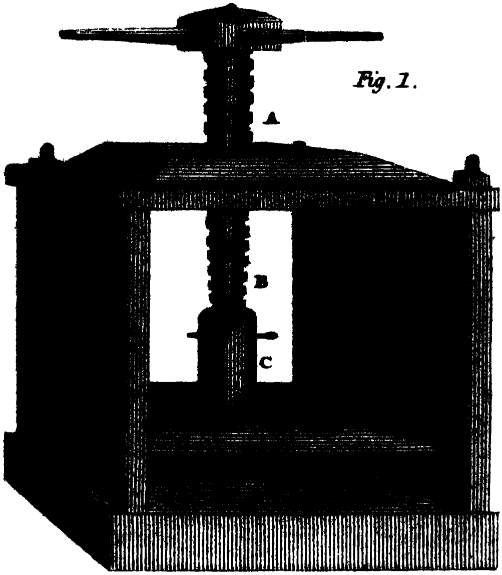



Fig. 1.

## Pump with double Piston.

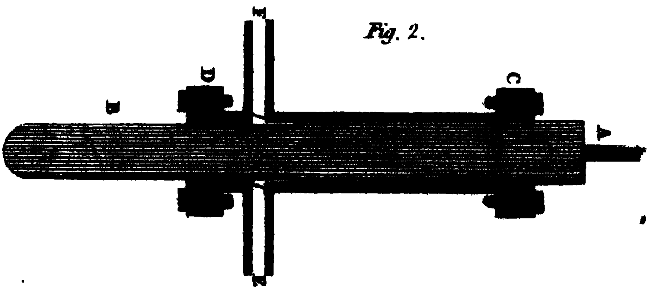



Fig. 2.

A

# JOURNAL

OF

# NATURAL PHILOSOPHY, CHEMISTRY,

AND

# THE ARTS.

*FEBRUARY*, 1804.

## ARTICLE I.

*Description of an Apparatus, by which the Effect of Atmospheric Pressure in supplying Worm-Tubs and other Vessels with Water on the Syphon Principle, may be secured against any Interruption to be caused by the extricated Air. In a Letter from Sir* A. N. EDELCRANTZ, *Counsellor of the Chancery, and Private Secretary to the King of Sweden, Member of the Swedish Academy, &c. &c.*

To Mr. NICHOLSON,

MY DEAR SIR,

Introduction respecting former endeavours to supply worm-tubs, &c. with water by the action of the syphon.

ON my late return from an agricultural tour in Scotland, I find you have in your excellent Journal, honoured me with the mention of some ideas of mine, respecting the application of syphons to raise water, with less expence than usual, for coolers in distilleries, condensers of steam-engines, &c. The use of syphons is so well and so generally understood, that though the most obvious things do not always present themselves first to the mind, it would have been surprizing if the same notion had not likewise occured to others. In fact, I have since learned, that a person in America, more than twenty years ago, obtained a patent for using the syphon to cool the worm-pipes in distilleries, and though I neither know his name, nor the specification of his invention, I presume it to

be fimilar to what I have propofed. In addition to which, the ingenious contrivance of Mr. Howard, publifhed in a later number of your Journal, is alfo founded upon the fame hydroftatical laws. But whatever may be the cafe in refpect to theory, no practical ufe feems yet to have been made of it, owing moft probably to unforefeen difficulties in the execution.

The chief impediment lies in the extrication of air which ftops the current.

The principal of thefe, in my opinion, as was mentioned in the former memoir upon this fubject, is the extrication of air from the water contained in the cooling veffel; which air being expanded, partly by its own elafticity under a diminifhed preffure, and partly by the heat, is accumulated in the upper part of the cooler, and interrupts the two columns of the fyphon, fo as at laft to ftop the motion entirely. For though it may poffibly happen, by the negligence of workmen, that the worm itfelf may not be fufficiently found to exclude the external air preffing into the cavity, and introducing itfelf that way into the cooler, as is ftated in the experiment of Mr. Howard; yet I am perfuaded that in moft inftances, this caufe may be taken for the real one, inftead of the former, which can never fail to act in proportion to the perpendicular height of the cooler, and the heat communicated by the worm.

Not probable that this air comes from without.

As it is impoffible to obferve by what actual means the air is produced, unlefs the whole apparatus were of glafs, fuch a miftake may eafily be made. In fact, if metallic pipes could not be made air-tight, even in a bended form, the exhauftions in air-pumps and ftill more in great condenfations * would be attended with almoft infuperable difficulties.

Plan for extracting it.

For extracting the air difengaged from the water itfelf, during the action of the fyphon, which in a few hours time would neceffarily interrupt its motion, I have employed a few hours of leifure conftructing the following means, which I beg leave, Sir, to communicate to you; without pretending to claim a greater portion of your attention than their remote utility may appear to deferve.

* In the experiments I made fome years ago, with an *apparatus* conftructed *for condenfing different gafes and for producing a ftronger artificial cold, than by any means employed before*, I compreffed air in brafs pipes more than to $\frac{1}{25}$ of its original volume, which if we fuppofe the cooler to be 15 feet high, is more than 50 times the difference between the external and internal preffure of air in the cooling worm of the new apparatus.

On

On the top of the cooling veſſel A, *Fig.* 1. *Plate* V. let a ſmall veſſel B, called the *air-veſſel*, be placed, communicating with A by a cock. With the top of B let another veſſel C, called the *water-veſſel* be connected, having a cock C. Theſe cocks have two parallel openings each, one for letting out air, the other for letting in water at the ſame time to take its place. Suppoſe theſe three veſſels to be filled with water and the ſyphon in action, the cock B open and C ſhut; then all the air produced in the cooler will riſe through B and fill the air-veſſel; and in the mean time the water will deſcend. The air-veſſel being filled with air, let the cock B be ſhut, and C opened; then the water from C will deſcend, and the air will eſcape into the atmoſphere. By this alternate motion of the cocks, the air diſengaged within the worm-tubs will be continually extracted, as long as any water remains in C.

A veſſel filled with water, with a cock at top and another at bottom, is connected with the upper part of the worm-tub. When the lower cock only is opened the extricated air aſcends, and water deſcends. When the upper cock only is opened, the air eſcapes, and other water fills in from above.

The principal object then muſt be firſt to ſupply the water in C, either by manual labour, or by a part of the mechanical force employed to fill the artificial reſervoir for the lower branch of the ſyphon; and ſecondly, to direct the alternative opening of the cocks, by a ſelf-regulating mechaniſm; which may be effected in the following manner.

The upper water muſt be ſupplied; and the cocks muſt work without attendance.

*Fig.* 2. *Plate* VI. repreſents a ſection of the *air-veſſel* B, B is the air and water-cock, communicating with the worm-tub or cooler below. A is the other cock for the ſame purpoſe, communicating with the water-veſſel C. A and B being connected by a metallic rod K L, their motions are ſimultaneous, and the perforations are ſo placed, that when the two holes of one cock are open, thoſe of the other are always ſhut. E is a ball of lead or other weight, fixed on the lever G U, connected with the quadrant F G, and the lever G H, the whole turning through one quarter of a circle round the centre U; *m n* is a part of a cog-wheel fixed to and ſliding along the quadrant G F, acting upon the teeth of the cock A, and forcing it, together with the cock B, one way or the other, as itſelf is moved by the levers U F or U G, either in the direction from F to G, or from G to F. This alternate motion is produced by the floater P O Q R, (of which a plan on a diminiſhed ſcale, is repreſented in *Fig.* 3.) having in its middle and hollow part a kind of fork O, through which paſſes the metallic rod H I, connected with the lever H F, at the end of the quadrant G F. This floater in its aſcenſion

The cocks are worked by a float: which when at loweſt ſhuts the under and opens the upper cock; and when at higheſt does the contrary. The action is made ſudden by a tumbling weight.

 lifts

lifts the little ball H at the end of the lever, and consequently the weight E; and in descending it acts upon the ball I, and forces the weight E in the contrary direction. A side view of this apparatus is presented in *Fig*. 4. where the double openings in the cocks A, B, and the manner of fixing the weight E with its levers, are more distinctly seen.

In *Fig*. 2. the air-vessel is supposed half filled with air transmitted from the cooler. The cock B is shut, and the cock A open, admitting water in place of the air. The floater is ascending, and when the fork O touches the ball H, the weight E is lifted, the quadrant begins to move from E to G, but the sliding part *mn* remains in its place till the weight E, having passed the perpendicular and falling over the other side, brings the point F in contact with *n*, and forces at once both cocks in the contrary direction; by which A being shut and B open, the air from the cooler rises, and the water descends. The floater consequently sinks till it presses upon I, drawing the weight E in its first situation, and turning the cocks again. This process will evidently continue to alternate.

Every mechanical reader will see the necessity of turning the cocks in this apparatus by a sudden stroke. For if the floater were to act directly and constantly upon them; there would be a certain moment when both would be shut, and consequently the motion would cease.

Another method by chains.

Instead of the slider *m n*, the same effect may be produced by two chains, (*Fig*. 5.) on the ends of the levers F G, connected with the cock A; the other apparatus remaining as described above. This method may even in some respects appear more simple; but I have described the other partly because I apprehend the contrivance to be new, and partly because the use of chains may in some cases be liable to objections.

An air-pump would be the simplest organ.

But I cannot avoid remarking that the most simple means of all, and perhaps the best, on a small scale, would be an *air-pump* fixed on the top of the cooler, and connected with the pump that supplies the artificial reservoir. And though the force required for extracting the rarified air, must be in proportion to the perpendicular height of the syphon; yet it cannot be more than what is required to raise a quantity of water necessary to fill the place of the air, as in the former

apparatus. When it has been found by experiments, how much air is extricated from a given quantity of water, running with a known velocity, under a given diminution of preſſure and heat; the ſize and power of the air-pumps may be determined ſo as to act with full advantage, as long as theſe circumſtances remain unvaried. But if they happen to change, the air-pump will either work too much, and expend an unneceſſary force, by extracting water after the air is exhauſted; or too little, by extracting the air more ſlowly than it is produced. On the contrary, it ſeems that the floater, if the veſſels and cocks be of ſufficient ſize, can never deliver more or leſs than is exactly required.

This conſideration, as well as the poſſibility of a conſtant ſupply of water for ſmall veſſels, without any pumping at all, may in many caſes allow the firſt apparatus to be uſed with advantage, and even render it preferable, eſpecially in elevated ſyphons and great rarefactions, as moſt commonly occur in practice, when it would not only be very difficulty to keep the air-pump tight without great friction, but a quantity of vapour from the heated fluid would be expanded. This increaſes the difficulty of the air-pump, but in the other contrivance it is partly condenſed by the cold water in the air-veſſel trough, which it is obliged to paſs. but would perhaps be very heavy on a ſmall apparatus.

I am with great eſteem, My dear Sir,

Your obedient humble Servant,

A. N. EDELCRANTZ.

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## II.

*Enquiries concerning the Nature of a Metallic Subſtance lately ſold in London as a new Metal, under the title of Palladium. By* RICHARD CHENEVIX, *Esq. F. R. S. and M. R. I. A.* *

ON the 29th of April I learned, by a printed notice † ſent to Mr. Knox, that a ſubſtance, which was announced as a new metal, Announce of a new metal for ſale.

* From the Philoſ. Tranſ. 1803.

† "Palladium, or new ſilver, has theſe properties amongſt others "that ſhew it to be a new noble metal.

"1. It diſſolves in pure ſpirit of nitre, and makes a dark-red "ſolution. 2. Green vitriol throws it down in the ſtate of a re-"gulus from this ſolution, as it always does gold from aqua regia.

"3. It

metal, was to be fold at Mr. Forfter's, in Gerrard-ftreet. The mode adopted to make known a difcovery of fo much importance, without the name of any creditable perfon except the vender, appeared to me unufual in fcience, and was not calculated to infpire confidence. It was therefore with a view to detect what I conceived to be an impofition, that I procured a fpecimen, and undertook fome experiments to learn it properties and nature.

Its general properties.

I had not proceeded very far, when I perceived that the effects produced by this fubftance, upon the various tefts, were fuch as could not be referred, *in toto,* to any of the known metallic fubftances. I immediately returned to Mr. Forfter, and became poffeffed of the whole quantity which had been left in his hands for fale. I could not obtain any information as to its natural ftate, or any trace that might lead to a probable conjecture.

Laminated fmall pieces.

The fubftance had been worked by art: it had been rolled out in flatting-mills; and was offered for fale in fpecimens confifting of thin laminæ. The largeft of them were about three inches in length, and half an inch in breadth, weighing on the average 25 grs. and were fold for one guinea. The other laminæ were fmaller, in proportion to the price.

Polifh, like platina: Elafticity moderate: Flexibility confiderable. Specific gravity, differing in the fpecimens.

Subjected to the fame treatment as platina, to procure a polifhed furface, palladium affumed an appearance fcarcely to be diftinguifhed from that metal. The laminæ were not very elaftic, but were very flexible, and could be bent feveral times in oppofite directions without breaking. The fpecific gravity, I found to differ not a little from that which is ftated in the printed notice, and to vary confiderably in different fpecimens. Some pieces of this fubftance were as low as 10,972, while others gave 11,482.

" 3. If you evaporate the folution, you get a red calx that diffolves " in fpirit of falt or other acids. 4. It is thrown down by quick- " filver, and by all the metals but gold, platina, and filver. 5. Its " fpecific gravity by hammering, was only 11.3; but by flatting, " as much as 11.8. 6. In a common fire the face of it tarnifhes " a little, and turns blue, but comes bright again, like other noble " metals, on being ftronger heated. 7. The greateft heat of a " blackfmith's fire would hardly melt it; 8. But if you touch it, " while hot, with a fmall bit of fulphur, it runs as eafily as zinc.

" It is fold only by Mr. Forfter, at No. 26, Gerrard-ftreet, " Soho, London; in famples of five fhillings, half a guinea, and " one guinea each."

The

The effects of galvanic electricity upon palladium, were the ſame as upon gold and ſilver. No oxidizement of the ſubſtance took place; but oxigen gas was emitted; during the whole time it formed a part of the galvanic circle in action. Galvanic effects, as gold or ſilver.

A lamina of this ſubſtance being expoſed to the blowpipe, the ſide removed from the immediate action of the flame became blue; but the temperature at which this colour was produced, exceeded that at which ſteel begins to loſe the tinge it had received at a lower heat. Became blue from heat.

I expoſed palladium, in an open veſſel, to a greater degree of heat than that which can melt gold. No oxidizement enſued; and, although the metallic ſlip was extremely thin, no appearance of fuſion took place, even at the edges or corners. Upon increaſing the fire conſiderably, I obtained a melted button; but I cannot eſtimate the degree at which the fuſion was effected. No oxidizement by ſtrong heat. Fuſion more difficult than that of gold.

The button, by this treatment, had loſt a little of its abſolute weight; but its ſpecific gravity had increaſed from 10,972 to 11,871. It was of a grayiſh-white. Its hardneſs was rather ſuperior to that of wrought iron. By the file, it acquired the colour and brilliancy of platina. It was malleable to a great degree. Its fracture was fibrous, and in diverging ſtriæ, which ſeemed to be compoſed of cryſtals; the ſurface of the button alſo, when ſeen through a lens, appeared to be cryſtallized. Fuſed button rather lighter, more denſe, harder than iron, malleable, fracture fibrous cryſtallized.

Palladium very readily combines with ſulphur. I expoſed a certain quantity of it to a violent heat, without being able to melt it; and, at that elevated temperature, threw ſome ſulphur upon it. It immediately entered into fuſion, and remained in that ſtate until the redneſs of the crucible was hardly viſible in the daylight. The increaſe of weight in the button of the ſulphuret, was ſuch as could not indicate with exactneſs the proportion of ſulphur combined with it; and I was ſo limited in the quantity of palladium I could obtain on any terms, that I thought it prudent to reſerve as much as poſſible for the inveſtigation of more important properties. Sulphuret of palladium is rather whiter than the ſubſtance itſelf, and is extremely brittle. Combines and flows with ſulphur by ignition. Sulphuret whiter and very brittle.

Palladium, melted in a charcoal crucible, and kept in fuſion for fifteen minutes, did not acquire any properties different from thoſe which I have already mentioned, in ſpeaking of the Continued fuſion of palladium in charcoal; no change.

effect

effect of heat upon that fubftance. Hence we may conclude, that there is not any action between charcoal and palladium.

Alloy. Palladium and gold; gray and lefs ductile.

I put equal parts of palladium and gold into a crucible, for the purpofe of forming an alloy. The refult, owing to an accident, did not weigh fo much as the fum of the quantities employed; therefore, the proportions in this alloy were uncertain. Its colour was gray; its hardnefs about equal to that of wrought iron. It yielded to the hammer; but was lefs ductile than each metal feparate, and broke by repeated percuffions. Its fracture was coarfe-grained, and bore marks of cryftallization. Its fpecific gravity was 11,079.

Palladium and platina; denfer, lefs malleable.

Equal parts of platina and palladium, entered into a fufion at a heat not much fuperior to that which was capable of fufing palladium alone. In colour and hardnefs, this alloy refembled the former; but it was rather lefs malleable. Its fpecific gravity, I found to be 15,141.

Palladium and filver: whiter.

Palladium, alloyed with an equal weight of filver, gave a button of the fame colour as the preceding alloys. This was harder than filver, but not fo hard as wrought iron; and its polifhed furface was fomewhat like platina, but whiter. Its fpecific gravity was 11,290.

Palladium and copper: harder dull colour.

The alloy of equal parts of palladium and copper was a little more yellow than any of the preceeding alloys, and broke more eafily. It was harder than wrought iron; and, by the file, affumed rather a leaden colour. Specific gravity 10,392.

Palladium and lead: more fufible, gray, clofe grained, and hard and brittle.

Lead increafes the fufibility of palladium. An alloy of thefe metals, but in unknown proportions, was of a gray colour, and its fracture was fine grained. It was fuperior to all the former in hardnefs, but was extremely brittle. I found its fpecific gravity to be 12,000.

Palladium and tin: grayifh, brittle, compact.

Equal parts of palladium and tin gave a grayifh button, inferior in hardnefs to wrought iron, and extremely brittle. Its fracture was compact and fine-grained. Specific gravity 8,175.

Palladium and bifmuth; gray, very hard and brittle.

With an equal weight of bifmuth, palladium gave a button ftill more brittle, and nearly as hard as fteel. Its colour was gray; but, when reduced to powder, it was much darker. Its fpecific gravity, I found to be 12,587.

Palladium and iron. Palladium and arfenic.

Iron, when alloyed with palladium, tends much to diminifh its fpecific gravity, and renders it brittle. Arfenic increafes the fufibility of palladium, and renders it extremely brittle.

From

From thefe experiments, we may form the following table, fhewing the difference between the true and the calculated mean of fpecific gravity in the alloys of palladium.

Comparifon of the fpecific gravities of thefe alloys with the computed denfities.

| | Metals. | Proportion. | Specific gravity by calculation *. | Specific gravity by experiment. | Difference. |
|---|---|---|---|---|---|
| Palladium alloyed with | Gold - | uncertain. | uncertain. | 11,079 | uncertain. |
| | Platina - | equal parts. | 17,241 | 15,141 | —2,100 |
| | Silver - | equal parts. | 10,996 | 11,290 | + ,294 |
| | Copper | equal parts. | 10,176 | 10,392 | + ,216 |
| | Lead - | equal parts. | uncertain. | 12,000 | uncertain. |
| | Tin - | equal parts. | 9,340 | 8,175 | —1,165 |
| | Bifmuth | equal parts. | 10,652 | 12,587 | + 1,935 |

Palladium fufed with potafh loft weight and brilliancy.

I expofed ten grains of palladium to the action of potafh, in fufion during half an hour. The fubftance loft its brilliancy, and diminifhed two grains and a half in weight; thefe were found in the potafh.

With foda.

The action of foda upon palladium, does not appear to be quite fo violent.

Ammonia in the humid way determines oxidation, and takes up oxide.

Ammonia, allowed to remain for fome days upon palladium, acquires a flight bluifh tinge, and holds a fmall portion of oxide of palladium in folution. In all thefe cafes, the action of the alkali is promoted by the contact of the atmofpheric air, the oxigen of which combines the metal, in favour of the affinity the oxide of palladium poffeffes towards the alkali.

Some of the pieces of palladium were more eafily acted upon by the acids than others; and, in general, thofe of the greateft fpecific gravity were the leaft affected. Upon the whole, however, the following ftatement may be taken as the average of the habitudes of palladium with the acid folvents.

* In the fpecific gravities of the different metals, I have followed the table given in our beft elementary work, Dr. Thompfon's Syftem of Chemiftry.

Sulphuric

Sulphuric acid and palladium. Weak action: beautiful red solution.

Sulphuric acid, boiled upon palladium, acquires a beautiful red colour, and dissolves a portion of the substance. The action of this acid is not very powerful, and, upon the whole, it cannot be looked upon as a good solvent for palladium.

Nitric acid more active; fine red solution.

Nitric acid acts with much greater violence upon palladium. It oxidizes the substance with somewhat more difficulty than it can oxidize silver; and, by dissolving the oxide, forms a very beautiful red solution. If the nitric acid be impregnated with nitrous gas, its action upon palladium is much more rapid.

Muriatic acid. Red solution.

Muriatic acid, by being boiled upon palladium for a considerable time, acts upon it, and becomes of a beautiful red.

Nitro-muriatic acid is the true solvent.

But the true solvent of palladium is nitro-muriatic acid, which attacks it with great violence, and forms a beautiful red solution.

Precipitate from acids by alkalis or earths, fine orange partly redissolved.

From all these acid solutions of palladium, a precipitate may be produced by the alcalis and earths. These precipitates are, for the most part, of a beautiful orange; are partly redissolved by some of the alcalis; and the supernatant liquor of the precipitate formed by ammonia is sometimes of a fine greenish blue. Sulphate, nitrate, and muriate, of potash, or of ammonia, produce an orange precipitate in the salts of palladium, as in those of platina, when not in too dilute solution; and the precipitates from the nitrate of palladium are in general of a deeper orange. All the metals, except gold, platina, and silver, cause very copious precipitates in solutions of palladium. Recent muriate of tin produces a dark orange or brown precipitate in neutralized salts of palladium, and is an extremely delicate test. Green sulphate of iron precipitates palladium in the metallic state; and, if the experiment succeed, the precipitate is about equal in weight to the palladium employed. Prussiate of potash causes an olive-coloured precipitate; and water impregnated with sulphuretted hidrogen gas, a dark brown one. Fluoric, arsenic, phosphoric, oxalic, tartaric, citric, and some other acids, together with their salts, precipitate some of the solutions of palladium, and form various combinations with this substance.

Such are the principal characters I have found in palladium, examined as a simple metallic body. It does not appear that, in stating any of its properties, except its specific gravity, the printed notice has been guilty of misrepresentation.

From

From theſe experiments, it would be difficult to ſay of what metal, or of what combination of metals, palladium conſiſts. We could not ſuppoſe gold or platina to be an ingredient in it, as it is in ſome meaſure acted upon by ſulphuric and muriatic acids, and is wholly ſoluble in nitric acid. Silver is excluded, by the effect of muriatic acid upon its ſolutions; as is lead, by that of the ſulphuric. Tin, antimony, biſmuth, or tellurium, would have left an inſoluble reſiduum with nitric acid. No traces could be found of any of the acidifiable metals; and iron was looked for with particular care, but in vain. In a word, the precipitation by the metals, ſeems to exclude all thoſe of eaſier oxidability than mercury; and this we ſhould not ſuppoſe to be preſent, as copper is not in the leaſt whitened, when uſed to precipitate palladium.

Compariſon of this body with the other known metals.

The ſtriking ſimilarity of many of the precipitates of palladium with thoſe of platina, induced me to multiply the comparative experiments; and I conſtantly obſerved contradictory facts. The ſpecific gravity, eaſy fuſibility, combination with ſulphur, precipitation by green ſulphate of iron and by pruſſiate of potaſh, together with other effects, were ſuch as I could not reconcile to the known characters of platina; unleſs I could ſuppoſe that a ſubſtance did exiſt, which could totally change its phyſical and chemical properties, or ſo diſguiſe them as to render them proof againſt the evidence of chemical re-agents.

It reſembles platina in many of its precipitates, &c.

The lighteſt of the metals is tellurium; yet, in order to produce an alloy of the ſpecific gravity of palladium, (ſuppoſing for a moment the real denſity of the alloy equal to the calculated mean), it would require two parts of tellurium and one of platina; and it is highly improbable, that ſo large a proportion of tellurium could exiſt in any maſs, without being detected. We have been told of very extraordinary anomalies in chemical affinities, by Mr. Berthollet; and Mr. Hatchett has made us acquainted with ſome, not leſs extraordinary, in the properties of alloys. Yet I think we ſhall ceaſe to wonder at what has been related by theſe chemiſts, when we learn that palladium is not, as was ſhamefully announced, a new ſimple metal, but an alloy of platina; and that the ſubſtance which can thus maſk the moſt characteriſtic properties of that metal, while it loſes the greater number of its own, is mercury.

*It is an alloy of platina and mercury.*

I confeſs it was not from an analyſis of palladium that I was firſt led to this reſult; for I had convinced myſelf, by ſyntheſis, of

This was ſynthetically diſcovered.

of its nature, and had formed the ſubſtance, before I could deviſe any probable method of aſcertaining its component parts.

Admirable diſpoſal of platina and mercury by which they were preſented to each other and united.—Green ſulphate of iron was added to a ſolution of platina; and alſo to a ſolution of mercury. No precipitate took place in either. The compounds were then mixed. Platina and mercury fell down together. The compound was fuſed. It was palladium.

In reflecting upon the various modifications which ſubſtances undergo when in union with each other, and on the variations produced in the laws of affinity by the intervention of new bodies, I was induced to try whether by the affinity of platina with ſome metal eaſily reduced, it might not happen that a reduction of both would take place by green ſulphate of iron, although no ſuch effect were produced upon each metal when ſeparate. The moſt likely to ſucceed, as being moſt eaſily reduced, after gold, platina, and ſilver, was mercury. I poured ſome ſolution of green ſulphate of iron into a ſalt of platina, and alſo into a ſalt of mercury; no precipitation took place I united the two liquors; and a precipitate exactly reſembling that which is formed by green ſulphate of iron in palladium, was inſtantly formed. I collected the precipitate, and expoſed it to a ſtrong heat; and, after repeated trials, obtained a metallic button, not to be diſtinguiſhed from palladium.

It certainly is one of the moſt extraordinary facts reſpecting alloys, that two metals, by their union with each other, ſhould ſo loſe the characteriſtic properties of each individually, that neither of them can be immediately detected by the uſual methods. Nothing but an affinity of the moſt powerful order could produce ſuch effects. But, to place the metals under the moſt favourable circumſtances for that affinity to exert its influence, and to promote their union, is not the reſult of common methods. Among a great number which I have tried, many have failed, and none have been attended with uniform ſucceſs. I have, however, formed palladium by the immediate union of platina and mercury; and, as whatever may place the apparent capricioufneſs of this combination in a conſpicuous point of view is not devoid of intereſt, I ſhall deſcribe the means by which I have attempted to produce it, whether they failed, or were attended with ſucceſs.

Palladium by the immediate union of platina and mercury.

### SYNTHETICAL EXPERIMENTS.

*Exp.* 1. It was not till after repeated trials of the mode juſt mentioned, that I ſucceeded in forming palladium. In many inſtances, I obtained a button completely melted, of the ſpecific gravity of 13, and ſometimes more; not ſo eaſily fuſed by ſulphur as palladium; not ſoluble in nitric acid; and the abſolute

abfolute weight of which exceeded that of the platina originally employed. But, although this fubftance was not platina, I could not fay it was palladium. The moft fuccefsful experiment by this method, was attended with the following circumftances. I diffolved one hundred grains of platina in nitro-muriatic acid, and then put in two hundred grains of red oxide of mercury, made by nitric acid; but this not being fufficient to faturate the excefs of acid, I continued to add more, until it ceafed to be diffolved. On the other hand, I prepared fome green fulphate of iron, and poured it into a long-necked mattrafs. I then poured the mixed folution of platina and mercury into the folution of green fulphate of iron, and heated the whole upon a fand bath. In lefs than half an hour, a copious precipitate was formed; and the infide of the mattrafs was lined with a thin metallic coat. The liquor was paffed through a filtre, which I had weighed; and the precipitate, after digeftion with muriatic acid, was well wafhed and dried. When I had collected as much of this as I could, there remained upon the filtre 12 grains; befides which, I had collected 264, in all 276. The fupernatant liquor ftill contained a portion of mercury, and about eight grains of platina. Therefore, the 276 were compofed of 92 of platina, and 184 of mercury. From this it appears, that one hundred grains of platina, can determine the precipitation of near two hundred grains of mercury, by green fulphate of iron; and that, in this proportion, there is a reciprocity of faturation. The 264, collected from the filtre, were expofed to a low red heat, and were reduced to 144. The twelve of the filtre would have given about feven; therefore, the whole would have been 151. The fubftance was in the form of a fine powder, and had a metallic luftre. It was then put into a charcoal crucible, and fufed into a button. This button weighed 128 grains, and with the quantity left on the fibre, would have weighed 135. In this 135, there were 92 of platina; therefore it was compofed of about two parts of that metal and one of mercury. It was of the fpecific gravity of 11,2; was wholly foluble in nitric acid; was eafily fufed by fulphur; was precipitated by green fulphate of iron: in a word, it was not to be diftinguifhed from palladium.

Experiment 1. The moft fuccefsful experiment of forming palladium by the mode firft mentioned.

It contained about two parts platina and one mercury.

*Exper.* 2. As another mode of forming palladium in the humid way, I put metallic iron into a mixed folution of platina and

Experiment 2. Mixed folution of platina and

mercury was precipitated by iron. The succefs was lefs complete.

and mercury. Both metals were precipitated; and the precipitate was fubmitted to the fame treatment as in the former cafe; but the fuccefs was not fo complete. Iron can precipitate either platina or mercury feparately; but green fulphate of iron can perform its function only in favour of the affinity of platina and mercury. Their union is promoted by its action; and the effects are, in all probability, fimultaneous. The combination of the metals takes place, if I may be allowed the expreffion, in their nafcent metallic ftate, and in a fixed proportion of mutual faturation. The union of the two metals, therefore, is in the prefent experiment lefs intimate, and the button which refults from fufing the precipitate, is of much greater denfity.

Exp. 3. Zinc the precipitant. Incomplete.

*Exper.* 3. The fame procefs was repeated, only ufing zinc inftead of iron, but the refult was not more fatisfactory.

Exp. 4. Mercury added to folution of platina. Incomplete.

*Exper.* 4. I poured fome mercury into a folution of platina, and heated them together for fome time. A precipitate took place; but, upon fufing it into a button, I did not find it to be palladium.

Exp. 5. Mixed folutions of platina and mercury evaporated and ignited. The mercury flew off.

*Exper.* 5. I diffolved the fame quantities of platina and mercury as in *Exper.* 1, in nitro-muriatic acid, and evaporated thofe folutions together. I then volatilized as much as I could of the mercury, at a red heat. At the end of the operation, I obtained precifely my original quantity of platina, reduced to the metallic ftate; but not one particle of the mercury remained along with it.

Exp. 6, 7. Platina and mercury precipitated by phofphate of ammonia. No fuccefs.

*Exper.* 6 and 7. The fame quantities of platina and mercury, diffolved in nitro-muriatic acid, were precipitated by phofphate of ammonia; and the liquor was evaporated. The refiduum, in a glaffy ftate, was expofed to a violent heat in a charcoal crucible; and I obtained a melted button, which weighed more than the original quantity of platina, and was of the fpecific gravity of 14,5. On account of the eafy fufibility of phofphuret of platina, I likewife tried to combine it directly with mercury, but could not fucceed.

Phofphuret of platina.

Exp. 8. Platina and mercury precipitated by fulphuretted hidrogen. In one inftance palladium.

*Exper.* 8. I precipitated a mixed folution of platina and mercury, by a current of fulphuretted hydrogen gas; and reduced the infoluble powder. After many attempts, in which I obtained buttons of the fpecific gravity of 14,3 and 14,5, I formed a piece weighing 11 grains, of the fpecific gravity of 11,5. This

11,5. This laſt was palladium; but I could not aſcertain the exceſs of weight, as a part of the original precipitate had been loſt.

*Exp.* 9. I mixed a ſolution of muriate of platina with pruſſiate of mercury, and obtained a ſlight precipitate. The liquor was evaporated, and the whole reſiduum expoſed to a violent heat. This experiment did not ſucceed. It was not repeated ſo often as the others; but I have ſome reaſon to think it might be attended with ſucceſs; for I obtained, in one inſtance, a few very minute grains, that were ſoluble in nitric acid.

Exp. 9. Muriate of platina and pruſſiate of mercury. Evaporation and ſtrong heat. Partial ſucceſs.

*Exp.* 10. I heated ſome purified platina, in the form of a very fine powder, with ten times its weight of mercury, and rubbed them together for a long time. The reſult was, an amalgam of platina. This amalgam, expoſed to a violent heat, loſt all the mercury it had contained; and the original weight of the platina remained without increaſe.

Exp. 10. Amalgam platina and mercury. Heat drove off the mercury.

*Exp.* 11. The beſt method of forming an amalgam of platina, is that preſcribed by Count Muſſin Puſhkin. I diſſolved a known quantity of platina in nitro-muriatic acid, precipitated by ammonia, and evaporated the liquor. The reſiduum was rubbed for a long time with a great quantity of mercury, and then expoſed to a violent heat. Many operations failed; in ſome, I had a button of the ſpecific gravity of 13,2. In one attempt, I ſucceeded completely: from 30 grs. of platina, treated as above, I obtained a button weighing 43,5, of the ſpecific gravity of 11,736, which had all the properties of palladium.

Exp 11. Count Muſſin Puſhkin's amalgam of platina heated gave palladium in one attempt.

*Exp.* 12. I fuſed together, in a charcoal crucible, 100 grs. of platina, 200 of cinnabar, 100 of lime, and 400 of calcined borax; and obtained a button, which weighed more than the platina, and was of the ſpecific gravity of 15,7. It was not ſoluble in nitric acid; but combined with ſulphur, at a red heat.

Exp. 12. Platina, cinnabar, lime and borax fuſed.

*Exp.* 13. In ſome experiments I had made, I found that the furnace in which I formed theſe alloys, was capable of melting platina, without the aſſiſtance of any flux except calcined borax. I therefore urged 100 grs. of platina, at a very ſtrong heat; and, when I judged the fire to have attained its greateſt intenſity, I poured mercury upon the platina, through a long earthen tube that terminated in the crucible,

Exp. 13. Platina fuſed and mercury thrown in. No effect.

and immediately withdrew the apparatus from the fire. No ſenſible union of the metals had taken place; nor had the platina increaſed in weight.

Exp. 14. Mercury in vapour paſſed over ignited platina. No effect.

*Exp.* 14. I put 100 grains of platina into an earthen tube, and placed the tube horizontally in the above furnace. At one end of it was a retort, containing 2lbs. of mercury. When the tube was at its greateſt heat, the mercury was made to boil; and the entire quantity paſſed over the ſurface of the platina, at that temperature. The experiment laſted one hour and a half; but the metals did not ſeem to have combined.

Exp. 15. Platina wire plunged in mercury and heated by galvaniſm. Little effect.

*Exp.* 15. Mr. Pepys was ſo obliging as to try the effect of his very powerful galvanic battery, in forming palladium. A piece of platina-wire was plunged into a baſon of mercury, and formed part of a galvanic circle. The wire was nearly in fuſion; but no combination ſeemed to take place. The nature of this experiment did not allow of very accurate weighing; but the fuſed globules of platina did not appear to have acquired the properties that conſtitute palladium.

Remarks. Diſpoſing affinities and aſſimilation.

Such are the experiments by which I attempted to form palladium. They were chiefly founded upon two principles; diſpoſing affinity, and aſſimilation. In the one caſe, I endeavoured to preſent to the metals that compoſe it, a ſubſtance which, on account of its affinity for ſome menſtruum neceſſary for their ſolution, and of their own tendency to combine in the proportions ſtated in *Exp.* 1, might cauſe them to unite in the form of an inſoluble compound. In the other caſe, I hoped to aſſimilate the properties of each, and, by making them ſomething more alike, to place them in the moſt favourable circumſtances for uniting. *Exp.* 1. was founded on the former, and *Exp.* 8 on the latter of theſe principles.

The platina always acquired weight when its properties were thus changed.

In many inſtances, when I did not form palladium, I obtained a metallic button which was not platina; and, when I did ſo, it always weighed more than the original quantity of platina employed. In repeating *Experiments* 1, 2, 4, 6, 8, 11, and 12, I ſeldom failed of having ſuch a ſubſtance. No effect of this kind took place in any experiment, when mercury was not uſed along with platina; and the other metals were merely acceſſaries, in promoting their union and precipitation. This is ſufficiently proved, by the uniformity of the reſults in different proceſſes, whether it was palladium or the ſubſtance I now mention which was formed. The chief property which

Mercury was indiſpenſable.

diſtinguiſhes

diftinguifhes the latter fubftance from platina, is its denfity. It is not unufual to obtain it of a fpecific gravity fo low as 13; very frequently 15 or 17. In the firft experiments, I fufpected this lightnefs to be owing to fome air-bubbles; but repeated fufion, and comparative experiments upon platina, foon convinced me of the contrary. The augmentation of weight alfo, which the platina never fails of acquiring, proves that this metal has combined with fome ponderable fubftance; and, in fact, the refult of thefe operations is, an alloy which is a mean betwixt platina in its pure ftate and what has been called palladium. It is, confequently, fubject to infinite variation. The firft effects which mercury produces upon platina are, to render it more fufible, and to diminifh its fpecific gravity. The next new property conferred upon it is, the power of uniting with fulphur; and, laftly, it becomes foluble in nitric acid. It is not however till the fpecific gravity is below 12, or 12,5 at moft, that it has acquired this property; and all thefe effects follow the direct order of the increafe of weight obfervable in the platina.

Mercury firft renders platina more fufible and lighter; then renders it combinable with fulphur; and ftill more renders it foluble in nitric acid, viz. under the fpecific gravity 12.

It is not very difficult to combine a fmall quantity of mercury with platina; but, to refolve the problem completely, and to produce an alloy of thefe metals which fhall be of fo low a fpecific gravity as 11,3, and fhall be foluble in nitric acid, is not fo eafily accomplifhed. From the repeated failures which I have experienced in thefe operations, I am much inclined to think that the author of palladium has fome method of forming it, lefs fubject to error than any I have mentioned. No doubt that perfeverance would put us in poffeffion of his fecret; but, being prevented by want of leifure from purfuing thefe refearches at prefent, I have confined myfelf to eftablifhing the fact, and defcribing the proceffes which I have employed.

The author of palladium probably has fome fteady method of compofing it.

Having thus acquired a certainty that mercury is a conftituent part of palladium, I made fome further experiments upon it, with a view to its analyfis; but they have not been attended with fo much fuccefs. It might be expected, from the great number of methods which have failed to form palladium, that many might be found to decompofe it when formed. But I have found the converfe of fuch proceffes as did not fucceed in producing palladium, to be ineffectual in deftroying the combination.

Its decompofition extremely difficult.

ANALYTICAL EXPERIMENTS.

Analytical experiments not fuccefsful.

*Exp.* 1, 2, and 3. The converfe of the fynthetical experiments 1, 2, 3, was made, but without any fatisfactory refult.

Excefs of mercury added to folution of palladium.

*Exp.* 4. The converfe of *Exp.* 4 was made without fuccefs. I put fome mercury into a folution of palladium, and left them together for fome time. The precipitate which was formed was palladium, juft as it had been ufed for the operation.

Palladium ftrongly and durably heated

*Exp.* 5. I expofed different pieces of palladium to a very violent heat for two hours. In fome, a diminution of abfolute weight, with an increafe of fpecific gravity, took place; in others, neither of thefe effects was produced. The fpecimens which I had made were chiefly of the latter kind.

Cupellation.

*Exp.* 6. Cupellation did not afford any fatisfaction refpecting the analyfis of palladium; but the heat neceffary for this purpofe is fo great, that I could not place great reliance upon this experiment. It is difficult to detach the button from the cupel with accuracy.

Combuftion in oxigen,

*Exp.* 7. I burned fome palladium in oxigen gas. A white fmoke arofe during the combuftion, and was depofited upon the fides of the glafs jar that contained the gas. But this fmoke was palladium, and not the mercury feparated from it.

by galvanifm.

*Exp.* 8. A flip of palladium, which Mr. Davy had the goodnefs to expofe, in my prefence, to the action of the ftrong galvanic batteries of the Royal Inftitution, burned with a very vivid light, and a white fmoke; but no mercury was feparated by this operation.

Wonderful differences between mixture and combination. Affinity not proportioned to the facility of union.

There is not any property of this compound which appears to me fo wonderful, as that which is manifefted by thefe experiments. It is a ftriking proof how unfounded was the opinion of fome philofophers, who fuppofed that the rapidity of combination was a meafure of the force of affinity. We do not know of any affinity among chemical bodies which is more powerful than that of platina and mercury appears to be. The obftacles which muft be overcome, in order to fix the latter metal, are a proof of this; yet the difficulty of forming this combination to its full extent is extreme. The difference which exifts between the compound and its elements, when merely mixed, either in folution or otherwife, cannot be better

better exemplified than by comparing the reſult of the 5th ſynthetical experiment, with the difficulty of expelling mercury from the compound.

*The mercury could not be ſeparated from the bought palladium; nor from any of the author's real combinations.*

I muſt here obſerve, that all the analytical experiments, and many others, were made, by way of compariſon, upon the palladium I had bought, as well as upon that which I had made. But, although I had myſelf combined the mercury with the platina, and conſequently knew it to be in the compound that reſulted, I could not ſucceed in ſeparating it. Neither did the ſubſtance deſcribed in a former paragraph, as intermediate between platina and palladium, allow one particle of mercury to eſcape from it, by any proceſs I have yet been able to deviſe.

*Objections to the word palladium.*

The name of palladium conveys to our mind the idea of ſomething abſolute, and therefore incapable of gradation. But gradations in alloys are infinite; and the alloy of platina and mercury is ſuſceptible of infinite variation. Palladium alſo brings to our recollection a contemptible fraud directed againſt ſcience: the name, therefore, ſhould not be admitted. I have called it an alloy; for it differs too much from the uſual idea we have annexed to the word amalgam, but it accurately correſponds with our notions of the name I have adopted.

*Theſe new facts are worthy of entire credit.*

The facts which I have related in this paper, appear at firſt ſight to have no ſimilar examples in chemiſtry; and may not gain immediate aſſent from every perſon. The philoſopher, indeed, will feel no humiliation in being forced to correct or to extend his knowledge; and will not altogether diſbelieve a fact, becauſe he can adduce no parallel inſtance, or becauſe it is not in uniſon with his received opinions. Such conduct would be raiſing an inſurmountable barrier againſt the progreſs of ſcience; it would be ſetting up our own feelings in the place of nature; and attempting to meaſure what in itſelf is immeaſurable, by the narrow ſcale of human comprehenſion.

But let us not confine our view of the facts and principles that have been mentioned, to this ſingle inſtance. Let us trace them in a more extended circle; and ſee whether any thing may be found in nature that can apply to the preſent ſubject.

*The ſtrange diminution of ſpecific gravity in this compound muſt not prejudice our belief.*

The firſt prejudice, for ſuch I muſt call it, againſt the preſence of platina in palladium is, the ſmall denſity of the alloy. And no doubt it is extraordinary, that a metal the ſpecific gravity of which is at leaſt 22, (Chabaneau ſays 24,) combined with another the ſpecific gravity of which is nearly 14,

ſhould produce a maſs of the ſpecific gravity of 10,972; not much more than half of that which calculation would denote, and inferior to either of its elements. In Mr. Hatchett's Paper upon the Alloys of Gold, to which I always refer with pleaſure, we find ſome extraordinary inſtances of anomalies in ſpecific gravity, both in exceſs and diminution upon the calculated mean. His experiments have not been doubted; nor can their accuracy be called in queſtion. The principle of deviation in the true and the calculated mean is therefore admitted. Who then can ſay where this deviation ſhall end, or mark out limits to the operations of nature?

Other ſimilar facts.

But a no leſs extraordinary inſtance of irregular denſity is daily before our eyes; yet it has not ſo much as attracted our attention. It is true that it is taken from among the gaſes. But, if we ſuppoſe that we have attained accuracy in experiments upon theſe ſubjects, I ſee no reaſon to refuſe their evidence in this inſtance. The denſity of oxygen gas, to that of water is as 1 to 740; and the denſity of hydrogen gas as 1 to 9792. The mean denſity of that proportion of oxygen and hydrogen gaſes which conſtitutes water, is to that of water as 1 to 2098; or, in other words, water is 2098 times heavier than the mean denſity of its elements in the gaſeous ſtate. But water is only 1200 times heavier than ſteam, or water in the ſtate of vapour. Therefore, there is a variation in +, of 898, or nearly half, between the denſity of water and its elements, when both are in the aeriform ſtate. This fact, however, regards bodies only as they remain in the ſame ſtate, whether of ſolidity, liquidity, or fluidity. The anomaly is much greater, if we contemplate them as they paſs from one of theſe ſtates to the other. Yet we muſt not omit the conſideration of ſuch a change, in the inſtance of mercury alloyed with platina; for the former metal, before liquid, becomes ſolid as it enters into the new combination.

Particularly the different ſpecific gravities of gaſiform water or ſteam and a mixture of the gaſes which form it. Theſe are as 7 to 4.

A ſtronger prejudice will perhaps exiſt againſt the fixation of ſo volatile a ſubſtance as mercury. It is certain that the labours of the alchemiſts have thrown ſome ridicule upon this ſubject, as a philoſophicial purſuit. Men of ſcience have long ſince declined the reſearch; and it is not probable that we are indebted to experiments undertaken in the true ſpirit of philoſophy, for the preſent fixation of mercury. But, the ſame cauſe which induced us to look upon the project as chimerical,

The fixation of mercury is a fact analogous to many others, and ought to excite no prejudice.

4 ſhould

ſhould diſpoſe us to admit it when accompliſhed. Every chemiſt well knows that ſimilar fixations of volatile ſubſtances are not uncommon If an ore containing ſulphur, or arſenic, or antimony, be gently roaſted, a great part of thoſe volatile bodies is driven off; but, if a fuſing heat be ſuddenly applied, the maſs unites in ſuch a manner that a very ſmall ſhare of them eſcapes. Mr. Hatchett has inſtanced an artificial combination of gold and arſenic, from which he could not expel the latter metal, by any degree of heat. Yet arſenic, though leſs fuſible, is not much leſs volatile than mercury. I will alſo add a caſe ſtill more in point; *viz.* the combination of arſenic and platina, which is not to be broken by a fuſing heat.

Inſtances; the volatile bodies ſulphur, arſenic, antimony.

An example of this fact, occurs again in water. The liquefaction or ſolidification of two gaſes to produce water, by a loſs of caloric, never ſhocks our mind, becauſe it is familiar to us. We cannot ſay what loſs of caloric may be ſuſtained by mercury, in order to unite with platina; or how far the preſence of the latter may contribute to expel caloric from the former. We know too, that at any temperature, without the aid of a combuſtible body, to act as a reductive, we have not been able to diſunite the laſt portions of oxygen, from the oxides of iron or of manganeſe. Yet, in the uſual method of reducing a metallic oxide, the oxygen is ſurrounded by a much greater quantity of caloric than is neceſſary to convert it into gas. Every fixation of a volatile ſubſtance is analogous to the preſent queſtion; and they whoſe minds have taken alarm from the novelty of the fact, may thus be familiarized with the neceſſity of admitting it.

Mercury having loſt caloric and combined with platina, may be in the ſame ſituation as oxygen in many oxides; viz. not expellable by mere heat, and perhaps by few affinities.

But, it may be objected, in the inſtances of iron or manganeſe, oxygen is combined with a combuſtible body, and retained in it by a decided and powerful affinity. There is no reaſon to ſuppoſe that ſuch an affinity may not exiſt among metals. We have been forced to acknowledge it, in a few caſes, among the earths; and, from the profound and ſagacious reſearches of Mr. Berthollet, we have learned many new facts, that promiſe us a rapid increaſe of knowledge. I ſhall beg leave to add a few examples, which are taken from that claſs of bodies to which the ſubject of the preſent Paper belongs, and ſhow that the metals obey the general law of mutual attraction.

Metals may combine together by an affinity comparable to that of a metal and oxygen.

*(To be concluded in our next.)*

# III.

*Inveſtigation of certain Theorems relating to the Figure of the Earth. By* JOHN PLAYFAIR, *F. R. S. Edin. and Profeſſor of Mathematics in the Univerſity of Edinburgh.* *

No complete reſults have yet been had as to the figure and magnitude of the earth.

1. THE obſervations which have been made to determine the magnitude and figure of the earth, have not hitherto led to reſults completely ſatisfactory. They have indeed demonſtrated the compreſſion or oblateneſs of the terreſtrial ſperoid, but they have left an uncertainty as to the quantity of that compreſſion, extending from about the one hundred and ſeventieth, to the three hundred and thirtieth part of the radius of the equator. Between theſe two quantities, the former of which is nearly double of the latter, moſt of the reſults are placed, but in ſuch a manner that thoſe beſt entitled to credit are much nearer to the leaſt extreme than to the greateſt. Sir Iſaac Newton, as is well known, ſuppoſing the earth to be of uniform denſity, aſſigned for the compreſſion at the poles $\frac{1}{230}$, nearly a mean between the two limits juſt mentioned; and it is probable, that, if the compreſſion is leſs than this, it is owing to the increaſe of the denſity towards the centre. Boſcovich, taking a mean from all the meaſures of degrees, ſo as to make the poſitive and negative errors equal, found the difference of the axes of the meridian $= \frac{1}{248}$. By comparing the degrees meaſured by Father Leiſganig in Germany, with eight others that have been meaſured in different latitudes, La Lande finds $\frac{1}{311}$, and, ſuppreſſing the degree in Lapland, which appears to err in exceſs, $\frac{1}{331}$ for the compreſſion. La Place makes it $\frac{1}{321}$; Sejour $\frac{1}{307}$, and, laſtly, Carouge and La Lande $\frac{1}{300}$.

Enumeration.

The reſults of obſerved eccentricity agree with the computations deduced from the length of the ſeconds pendulum.

Theſe reſults, which reduce the eccentricity of the meridians ſo much lower than was once ſuppoſed, agree well with the obſervations of the length of the pendulum made in different latitudes. Were the earth a homogeneous body, Sir Iſaac Newton demonſtrated, that the diminution of gravity under the equator would be $= \frac{1}{230}$, expreſſed by the ſame fraction with the compreſſion at the poles. M. Clairault made afterwards a very important addition to this theorem: for he ſhewed, that, if the earth be not homogeneous, but have a

* Edinburgh Tranſ. Vol. V.

denſity

denſity that varies with any function of the diſtance from the centre, the two fractions, expreſſing the compreſſion at the poles, and the diminution of gravity at the equator, when added together, muſt be of the ſame amount as in the homogeneous ſpheroid, that is, muſt be $= \frac{2}{230}$ or $\frac{1}{115}$. Now, the ſecond pendulum is concluded, from the beſt and moſt recent obſervations, to be longer at the pole than at the equator by $\frac{1}{185}$, and this, taken from $\frac{1}{115}$, leaves $\frac{1}{304}$ for the compreſſion at the poles.

Why have not the obſervations of axes agreed better?

2. But though $\frac{1}{300}$, or ſome fraction not very different from it, ſhould be admitted as the moſt probable value of the compreſſion, or ellipticity, as it is called, of the terreſtrial ſpheroid, it ſtill remains to be explained, why all the obſervations, conſidering the care with which they have been made, do not agree more nearly with this concluſion. Among the cauſes that may be aſſigned for this inconſiſtency, though unavoidable miſtakes, and the imperfection of inſtruments, muſt come in for a part, there can be little doubt that local irregularities in the direction of gravity have had the greateſt ſhare in producing it. Of theſe irregularities, that which ariſes from the attraction of mountains has had its exiſtence proved, and its quantity, in one caſe, aſcertained, by the very accurate obſervations of the preſent aſtronomer-royal at Schehallien in Perthſhire. We may trace the operation of this cauſe in many of the degrees that have been actually meaſured. Thus, in the degree at Turin, when divided into two parts, and each eſtimated ſeparately, that which was to the north of the city, and pointed toward Monte Roſa, the ſecond of the Alps in elevation, and the firſt perhaps in magnitude, was found greater in proportion than that toward the ſouth, the plummet having been attracted by the mountain above mentioned, and the zenith made of conſequence to recede toward the ſouth. There are no doubt ſituations in which the meaſurement of a ſmall arch might, from a ſimilar cauſe, give the radius of curvature of the meridian infinite, or even negative.

Anſ.—Principally becauſe the attractions of mountains have cauſed error in the uſe of the plumb-line.

Another more formidable cauſe of error:

But there is another kind of local irregularity in the direction of gravity, that may alſo have had a great effect in diſturbing the accuracy of the meaſurement of degrees. The irregularity I mean is one ariſing from the unequal denſity of the materials under and not far from the ſurface of the earth; and this cauſe of error is formidable, not only becauſe it may go to a great extent,

extent, but becaufe there is not any vifible mark by which its exiftence can always be diftinguifhed. The difference between the primary and fecondary ftrata is probably one of the chief circumftances on which this inequality depends. The primary ftrata, efpecially if we include among them the granite, may often have three times the fpecific gravity of water, whereas the fecondary, fuch as the marly and argillaceous, frequently have not more than twice the fpecific gravity of that fluid. Suppofe, then, that a degree is meafured in a country where the ftrata are all fecondary, and happens to terminate near the junction of thefe with the primitive or denfer ftrata, the line of which junction we fhall fuppofe to lie nearly eaft and weft; the fuperior attraction of the denfer ftrata muft draw the plummet toward them, and make the zenith retire in the oppofite direction; thus diminifhing the amplitude of the celeftial arch, and increafing, of confequence, the geodetical meafure affigned to a degree. From fuppofitions, no way improbable, concerning the denfity and extent of fuch maffes of ftrata, I have found, that the errors, thus produced, may eafily amount to ten or twelve feconds.

*that the denfity of the ftrata near the earth's furface muft be much greater towards fome regions than others.*

*This error may be ten or twelve feconds.*

3. While we continue to draw our conclufions, about the figure of the earth, from the meafurement of fingle degrees, there appears to be no way of avoiding, or even of diminifhing, the effects of thefe errors. But if the arches meafured are large, and confift each of feveral degrees, though there fhould be the fame error in determining their celeftial amplitudes, the effect of that error, with refpect to the magnitude and figure of the earth, will become inconfiderable, being fpread out over a greater interval; and it is, therefore, by the comparifon of two fuch arches that the moft accurate refult is likely to be obtained. But, in purfuing this method, fince the arches meafured cannot be treated as fmall quantities, or mere fluxions of the earth's circumference, the calculation muft be made by rules quite different from thofe that have been hitherto employed. Thefe new rules are deduced from the following analyfis.

*Accurate refults are therefore only to be had from large arcs;*

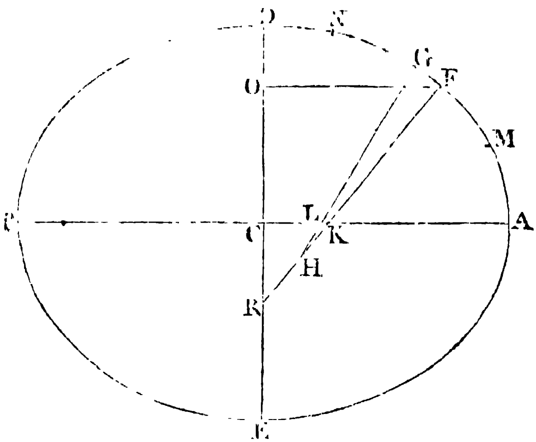
*and thefe require new modes of computation.*

4. Let the ellipfis ADBE reprefent a meridian paffing through the poles D and E, and cutting the equator in A and B. Let C be the centre of the earth, AC, the radius of the equator, $= a$, and DC, half the polar axis, $= b$. Let FG be any very fmall arch of the meridian, having its

*Inveftigation of formulas for computing the figure of the earth.*

its centre of curvature in H; join HF, HG cutting AC in K and L. Let $\phi$ be the meafure of the latitude of F, or the meafure of the angle AKF, expreffed, not in degrees and minutes, but in decimals of the radius 1; then the excefs of the angle ALG above AKF, that is, the angle LHK or GHF will be $= \dot{\phi}$, and therefore FG $= \dot{\phi} \times$ FG. Alfo, if the elliptic arch AF $= z$, FG $= \dot{z} = \dot{\phi} \times$ FH.

Inveftigation of formulas for computing the figure of the earth.




But FH, or the radius of curvature at F, is $=$

$$\frac{a^2 b^2}{(a^2 - a^2 \text{ fin }^2\phi + b^2 \text{ fin }^2\phi)^{\frac{3}{2}}} = a^2 b^2 (a^2 - a^2 \text{ fin }^2\phi + b^2 \text{ fin }^2\phi)^{-\frac{3}{2}},$$

as is demonftrated in the conic fections. Therefore, if $c$ be the compreffion at the poles, or the excefs of $a$ above $b$, $b^2 = a^2 - 2ac + c^2$, or becaufe $c$ is fmall in comparifon of $a$, if we reject its powers higher than the firft, $b^2 = a^2 - 2ac$, and FH $= a^3 (a - 2c)(a^2 - a^2 \text{ fin }^2\phi + a^2 \text{ fin}^2 \phi - 2ac \text{ fin }^2\phi)^{-\frac{3}{2}} = a^3 (a - 2c)(a^2 - 2ac \text{ fin }^2\phi)^{-\frac{3}{2}}$.

But $(a^2 - 2ac \text{ fin }^2\phi)^{-\frac{3}{2}} = a^{-3}(1 - \frac{2c}{a} \text{ fin }^2\phi)^{-\frac{3}{2}}$ $= a^{-3}(1 + \frac{3c}{a} \text{ fin }^2\phi)$ nearly, rejecting, as before, the terms that involve $c^2$, &c. Hence FH $= (a - 2c)(1 + \frac{3c}{a} \text{ fin }^2\phi) = a - 2c + 3c \text{ fin }^2\phi$.

Now

Inveſtigation of formulas for computing the figure of the earth.

Now $\dot{z} = \dot{\phi} \times$ FH, therefore $\dot{z} = \dot{\phi}\,(a - 2c + 3c \text{ ſin }^2\phi)$ $= (a - 2c)\dot{\phi} + 3c\dot{\phi} \text{ ſin}^2\phi$. But ſin $^2\phi = \frac{1 - \text{coſ}\,2\phi}{2}$, therefore $\dot{z} = (a - 2c)\,\dot{\phi} + \frac{3}{2}c\dot{\phi} - \frac{3c\dot{\phi}}{2}\text{coſ}\,2\phi$, and taking the fluent $z = \left(a - \frac{c}{2}\right)\phi - \frac{3c}{4} \text{ ſin } 2\phi$. To this value of $z$ no conſtant quantity is to be added, becauſe it vani ſhes when $z = o$.

Therefore an arch of the meridian, extending from the equator to any latitude $\phi$, is $= a\phi - \frac{c}{2}\left(\phi + \frac{3}{2} \text{ ſin } 2\phi.\right)$.

5. This theorem is alſo eaſily applied to meaſure an arch of the meridian, intercepted between any two parallels of the equator.

Thus, if MN be any arch of the meridian, $\phi'$ the latitude of M, one of its extremities, and $\phi''$ that of N, its other extremity, we have AM $= a\phi' - \frac{c}{2}\left(\phi' + \frac{3}{2} \text{ ſin } 2\phi'\right)$, and

AN $= a\phi'' - \frac{c}{2}\left(\phi'' + \frac{3}{2} \text{ ſin } 2\phi''\right)$. therefore the arch

MN $= a\,(\phi'' - \phi') - \frac{c}{2}\left((\phi'' - \phi') + \frac{3}{2} \text{ ſin } 2\phi'' - \frac{3}{2} \text{ ſin } 2\phi'\right)$.

6. If, therefore, MN be an arch of ſeveral degrees of the meridian, the length of which is known by actual meaſurement, and alſo the latitude of its two extremities M and N, this laſt formula gives us an equation, in which $a$ and $c$ are the only unknown quantities. In the ſame manner, by the meaſurement of another arch of the meridian, an equation will be found, in which $a$ and $c$ are likewiſe the only unknown quantities. By a compariſon, therefore, of theſe two equations, the values of $a$ and $c$, that is, of the radius of the equator, and its exceſs above half the polar axis, may be determined.

Thus, if $l$ be the length of an arch meaſured, $m$ the co-efficient of $a$, and $n$ of $c$, computed by the laſt formula; and if $l'$ be the length of any other arch, $m'$ the coefficient of $a$, and $n'$ of $c$, computed in the ſame manner, we have $ma - nc = l$,

and $m'a - n'c = l'$.

Whence $a = \frac{n'l - nl'}{mn' - m'n}$, $c = \frac{m'l - ml'}{mn' - m'n}$ and $\frac{c}{a} = \frac{m'l - ml'}{n'l - nl'}$. It

may

may be uſeful, in the numerical calculation, to obſerve alſo that $c = \frac{ma - l}{n}$.

7. The arch of the meridian, which was meaſured in Peru, compared with that meaſured in France, will afford an example of the application of theſe formulas.

Application of the formulas to the arc meaſured in Peru and that in France.

The amplitude of the arch meaſured in Peru was 3° 7′ 1″, and its length 176940 toiſes. To reduce this to the level of the ſea, above which it was elevated 1226 toiſes, 66 toiſes muſt be ſubtracted, and again 12 toiſes added to adapt it to the mean temperature of the atmoſphere. Thus corrected it is 176886 toiſes. The arch meaſured begun 36″ north of the equator, and extended to the parallel of 3° 6′ 25″ ſouth; we ſhall ſuppoſe it to have begun under the equator, and to have extended to the parallel of 3° 7′ 1″, a ſuppoſition which can produce no ſenſible error, and will ſomewhat ſimplify the calculation. Thus $\phi$, in the preceding formula, is an arch of 3° 7′ 1″ expreſſed in decimals of the radius 1, and ſo we have $m =$ .0544009, $n =$ .1086408, and $l =$ 176886.

Again, the amplitude of the whole arch meaſured in France from Dunkirk to Perpignan is 8° 20′ 2″½ and its length 475496 toiſes. The northern extremity of this arch is in latitude 51° 2′ 1″, and the ſouthern in 42° 41′ 58″½. Hence $\phi'' =$ .8907045, and $\phi' =$ .7452459, and therefore $m' =$ .1454586, $n' =$ .0585735, $l =$ 475496.

Therefore, $a = \frac{n'l - nl'}{mn' - m'n} =$ 3273325 toiſes;

$c = \frac{m'l - ml'}{mn' - m'n} =$ 10917 toiſes,

and $\frac{c}{a} = \frac{1}{300}$ nearly.

Wherefore alſo the longer axis of the meridian is to its conjugate, or $a$ is to $b$ as 300 to 299.

This proportion agrees well with that which was already pointed out as the moſt probable reſult, from the compariſon of ſingle degrees, and from obſervations of the pendulum. As theſe concluſions are obtained by different methods, they tend greatly to confirm one another.

8. From this too it ſeems highly probable, that the uncertainty which yet remains with reſpect to the true figure of the earth

Probability that the figure of the earth will be well determined

by meaſurement of ſome other conſiderable arcs.

earth will be entirely removed by the meaſurement of ſome other conſiderable arches of the meridian. Such an arch will be furniſhed by the ſurvey of Great Britain begun by General Roy, and ſtill continued in a ſtyle of accuracy ſo much ſuperior to any other ſyſtem of geometrical operations that has ever yet been executed. In drawing the concluſions from obſervations made with ſuch exactneſs, it may be neceſſary to employ a more accurate approximation than has been done in the preceding *formulæ*, by retaining the ſecond power of $c$. The equations to be reſolved will thus become of the ſecond order, but as the unknown quantities can be nearly found by the ſolution of a ſimple equation, the farther approximation to their true values will be accompanied with no difficulty.

Farther approximation to be then employed.

9. Concerning this farther approximation it may be uſeful however to remark, that if $c^2$ be retained, its coefficient in the formula of § 4 will be $\frac{1}{16a}\left(\phi + \frac{15}{4} \text{ ſin } 4\phi\right)$;

and therefore in the formula of § 5 it will be

$\frac{1}{16a}\left(\phi'' - \phi' + \frac{15}{4} (\text{ſin } 4\phi'' - \text{ſin } 4\phi')\right)$.

If then the quantity

$\frac{1}{16a}\left(\phi'' - \phi' + \frac{15}{4} (\text{ſin } 4\phi'' - \text{ſin } 4\phi')\right)$,

computed for any arch of the meridian, be put $= g$, and the ſame, computed for any other arch, be $= g'$, the equations of § 6 will become,

$$ma - nc + \frac{gc^2}{a} = l, \text{ and}$$

$$m'a - n'c + \frac{g'c^2}{a} = l'.$$

10. Here if we put $d$ for the value of $a$, as given by the formula $\frac{n'l - nl'}{mn' - m'n}$; and $h$ for the value of $c$, as given by the formula $\frac{m'l - ml'}{mn' - m'n}$, alſo $v$ for the correction to be made on $d$, and $u$ for the correction to be made on $h$, ſo that $a = d + v$, and $c = h + u$, by ſubſtituting theſe values of $a$ and $c$ in the two laſt equations we have $mv - nu + \frac{g(h+u)^2}{d+v} = 0$, and

$$m'v - n'u + \frac{g'(h+u)^2}{d+v} = 0.$$

Hence

Farther approximation to be then employed.

Hence, rejecting all the terms that involve $v^2$, $u^2$, or $uv$, we have $dmv - dnu + gh^2 + 2ghv = 0$,
and $dm'v - dn\,u + g'h^2 + 2g'hv = 0$.

Therefore, $v = \frac{(ng' - n'g)\,h^2}{(n'm - nm')\,d + (gn' - g'n)\,2h}$, also

$$u = \frac{g'h^2\,(dm + 2gh) - gh^2\,(dm' + 2g'h)}{dn'\,(dm + 2gh) - dn\,(dm' + 2g'h)}$$

And again, by rejecting those terms that are small in comparison of the rest, $v = \frac{h^2\,(ng' - n'g)}{d\,(n'm - nm')}$, and

$$u = \frac{h^2\,(g'm - gm)}{d\,(n'm - nm')}.$$

Thus $v$ and $u$ are found, and of consequence $d+v$ and $h+u$, that is $a$ and $c$, without neglecting any terms that are not of an order less than $\frac{c^2}{a}$; and when it is considered that

$\frac{c^2}{a}$

$\frac{c^2}{a}$ is less than $\frac{1}{22500}$, it will readily be allowed that it is quite unnecessary to carry the approximation farther.

The errors of observation are less the larger the arcs.

11. The same thing that renders the comparison of large arches of the meridian useful for lessening the effect of errors arising from irregularities in the direction of gravity, makes it serve to diminish the effect of all the errors of the astronomical observations at the extremities of the arches, from whatever cause they arise. They are all diffused over a greater interval, and have an effect proportionally less in diminishing the accuracy of the last conclusion.

Extensive surveys particularly useful.

12. The measurement therefore of large arches of the meridian, especially if performed in distant countries, is likely to furnish the best *data* for ascertaining the true figure of the earth; and on this account extensive and accurate surveys, such as that above mentioned, are no less interesting to science, in general, than conducive to national utility. The survey of this island, when completed, will furnish an arch of the meridian, beginning at the same parallel where that measured in France terminates, and nearly of the same extent, so that the length of an arch of more than 16°, or almost a twentieth of the earth's circumference, will become known. The different portions of this arch compared with one another, or with the arch

arch meafured in Peru, will afford a variety of *data* for determining the true figure of the earth.

Obfervations of perpendiculars to the meridian, and of parallels of latitude.

But furveys of the kind now referred to, afford likewife other materials from which the folution of this great geographical problem may be deduced. Thefe are chiefly of two forts, viz. the magnitude of arches, either of the curves perpendicular to the meridian, or of the circles parallel to the equator. Examples of the firft of thefe have been given by General Roy and Mr. Dalby; the obfervations which follow are directed toward both.

Large perpendiculars are of very difficult meafurement; and fmall ones erroneous from partial gravitation.

13. With refpect to the meafurement of arches perpendicular to the meridian, it may be obferved, that the directions of gravity at different points of fuch arches do not interfect one another at all, unlefs the diftances of thofe points from the faid meridian be very fmall. On this account the meafurement of a large arch perpendicular to the meridian would involve in it confiderable difficulty; to avoid which it is neceffary that the arch meafured be but fmall, or one that does not greatly exceed a fingle degree. Such meafurements are of courfe obnoxious to all the errors that arife from the deflection of the plumb-line, and cannot therefore furnifh *data* for determining the figure of the earth, equally valuable with thofe which may be derived from large arches of the meridian. The method of determining the figure of the earth, from degrees of the perpendicular to the meridian, is not however without its advantages, and in certain circumftances is preferable to any other that proceeds by the meafurement of arches equally fmall. This method is twofold; as a degree of the meridian may be compared with a degree of the perpendicular to it in the fame latitude; or two degrees perpendicular to the meridian, in different latitudes, may be compared with one another. The advantages peculiar to each will appear from the following inveftigation.

To find the axes of a fpheroid from comparing a deg. of the merid. with one of the perpend.

14. Let it be required to find the axes of an elliptical fpheroid, from comparing a degree of the meridian in any latitude with a degree of the curve perpendicular to the meridian in the fame latitude.

Let the ellipfis ADBE (Fig. on page 105) reprefent a meridian, of which a degree is meafured at F. Let the perpendicular

dicular to the meridian in F meet the lefs axis DE in R. Then R will be the centre of curvature of the circle cutting the meridian at right angles in F; for at any point in that circle indefinitely near to F, the direction of the plumb-line, or of gravity, as it always paffes through the axis DE, will cut DE in R; it will therefore alfo interfect FR in R, fo that R is the centre, and FR the radius, of curvature of the perpendicular to the meridian. Let H be the centre of curvature of the meridian itfelf at F: draw FO perpendicular to DE, and let the latitude of F, or the angle OFR $= \phi$. Alfo let AC $= a$, CD $= b$, and $a - b = c$, as before.

To find the axes of a fpheroid from comparing a deg. of the merid. with one of the perpend.

Then from the nature of the ellipfis, FO $=$ $\dfrac{a^2 \text{ cof } \phi}{\sqrt{a^2 \text{ cof }^2\phi + b^2 \text{ fin } \phi^2}}$, and becaufe fin FRO : 1 :: FO : FR, that is, cof $\phi$ : 1 : : FO : FR, FR $= \dfrac{a^2}{\sqrt{a^2 \text{ cof }^2\phi + b^2 \text{ fin } \phi^2}}$; and this, therefore, is the radius of curvature of the fection of the fpheroid perpendicular to the meridian at F. But the radius of curvature of the meridian at F, that is FH $=$ $\dfrac{a^2\, b^2}{\sqrt{a^2 \text{ cof }^2\phi + b^2 \text{ fin }^2\phi}}$, therefore

$$\text{FR} : \text{FH} :: \frac{a^2}{(a^2 \text{ cof}\,\phi^2 + b^2 \text{ fin}\,\phi^2)^{\frac{1}{2}}} : \frac{a^2\, b^2}{(a^2 \text{ cof}\,\phi^2 + b^2 \text{ fin}\,\phi^2)^{\frac{3}{2}}},$$

and dividing both by $\dfrac{a^2}{(a^2 \text{ cof}\,\phi^2 + b^2 \text{fin}\,\phi^2)^{\frac{1}{2}}}$, we have

FR : FH : : $a^2$ cof $\phi^2 + b^2$ fin $\phi^2$ : $b^2$,

15. If then D be the length of a degree of the meridian at F and D′ the length of a degree of the circle at right angles to it,

D′ : D : : $a^2$ cof $\phi^2 + b^2$ fin $\phi^2$ : $b^2$, and $\dfrac{\text{D}'}{\text{D}} =$

$$\frac{a^2 \text{ cof } \phi^2 + b^2 \text{ fin } \phi^2}{b^2} = \frac{a^2}{b^2} \text{ cof } \phi^2 + \text{fin }^2\phi. \text{ Hence}$$

$$\frac{\text{D}'}{\text{D}} - \text{fin } \phi^2 = \frac{a^2}{b^2} \text{ cof } \phi^2 \text{ and } \frac{a}{b} = \frac{\sqrt{\frac{\text{D}'}{\text{D}} - \text{fin } \phi^2}}{\text{cof } \phi}.$$

This laft formula, therefore, gives the ratio of $a$ to $b$ when D, D′ and $\phi$ are known.

To find the axes of a spheroid from comparing a deg. of the merid. with one of the perpend.

16. To find $a$ and $b$ themſelves, if $m = 57.2957$, &c. or the number of degrees in the radius, ſo that $mD' = FR = \frac{a^2}{(a^2 \operatorname{coſ} \phi^2 + b^2 \operatorname{ſin} \phi^2)^{\frac{1}{2}}}$, and ſince it has been already ſhewn that $\frac{a^2}{b^2} = \frac{\frac{D'}{D} - \operatorname{ſin} \phi^2}{\operatorname{coſ} \phi^2}$, or $b^2 = \frac{a^2 \operatorname{coſ} \phi^2}{\frac{D'}{D} - \operatorname{ſin} \phi^2}$, therefore

$$mD' = \frac{a^2}{\left(a^2 \operatorname{coſ} \phi^2 + \frac{a^2 \operatorname{coſ} \phi^2}{\frac{D'}{D} - \operatorname{ſin} \phi^2} \times \operatorname{ſin} \phi^2\right)^{\frac{1}{2}}} = \frac{a}{\operatorname{coſ} \phi \left(1 + \frac{\operatorname{ſin} \phi^2}{\frac{D'}{D} - \operatorname{ſin} \phi^2}\right)^{\frac{1}{2}}} \text{ and } a = mD' \operatorname{coſ} \phi \sqrt{1 + \frac{\operatorname{ſin} \phi^2}{\frac{D'}{D} - \operatorname{ſin} \phi^2}}.$$

$$\text{Now, } 1 + \frac{\operatorname{ſin} \phi^2}{\frac{D'}{D} - \operatorname{ſin} \phi^2} = \frac{\frac{D'}{D}}{\frac{D'}{D} - \operatorname{ſin} \phi^2} = \frac{1}{1 - \frac{D}{D'} \operatorname{ſin} \phi^2},$$

therefore $a = \frac{mD' \operatorname{coſ} \phi}{\sqrt{1 - \frac{D}{D'} \operatorname{ſin} \phi^2}}$.

17. This value of $a$ is very convenient for logarithmical calculation; for if $\operatorname{ſin} \phi \sqrt{\frac{D}{D'}}$ be computed, it will always be leſs than 1, becauſe $D'$ is greater than $D$, and therefore may be taken for the ſine of an arch $\psi$, of which arch $\sqrt{1 - \frac{D}{D'} \operatorname{ſin} \phi^2}$ will of courſe be the coſine, ſo that $a = \frac{mD' \operatorname{coſ} \phi}{\operatorname{coſ} \psi}$.

The ſame method may be uſed for finding $\frac{a}{b}$ from the formula in § 15.

In

To find the axes of a spheroid from comparing a deg. of the merid. with one of the perpends.

In the ſame manner that $a$ has been found, we will obtain $b = \frac{mD' \operatorname{coſ} \phi^2}{\left(1 - \frac{D}{D'} \operatorname{ſin} \phi^2\right)\sqrt{\frac{D'}{D}}}$.

If we examine theſe formulas in the extreme caſes, viz. when $\phi = 90°$, and when $\phi = 0$, we ſhall have in the former caſe $a = \frac{0}{0}$, becauſe coſ $\phi = 0$, and alſo $D' = D$, ſo that $1 - \frac{D}{D'} \operatorname{ſin}^2\phi = 0$. Here therefore $a$ is indefinite, and may be of any magnitude whatever; and it is evident that this is the reſult which the formula ought to give: becauſe at the pole, or when $\phi = 90°$, the perpendicular to the meridian is itſelf a meridian, and therefore the meaſurement of the two degrees, D and D′, is but the ſame with the meaſurement of one degree.

When $\phi = 0$, that is at the equator, the circle perpendicular to the meridian is the equator itſelf, and we have then $a = mD'$, $a$ being determined in this caſe by the degree of the equator alone. Here alſo we have $\frac{a}{b} = \sqrt{\frac{D'}{D}}$, which is known to be true.

18. The preceding formulas may be rendered more ſimple, if we aim only at an approximation, which indeed is all that is neceſſary in this inquiry. Since $c$ denotes the compreſſion, or ſince $a - c = b$, and therefore $a^2 - 2ac = b^2$ nearly, conſequently the radius of curvature of the meridian at F, that is $mD = \frac{a^2 (a^2 - 2ac)}{(a^2 - 2ac \operatorname{ſin} \phi^2)^{\frac{3}{2}}} = \frac{a^3 (a - 2c)}{a^3 \left(1 - \frac{2c}{a} \operatorname{ſin} \phi^2\right)^{\frac{3}{2}}} = (a - 2c)\left(1 - \frac{3c}{a} \operatorname{ſin} \phi^2\right)$, or $mD = a - 2c + 3c \operatorname{ſin} \phi^2$. In the ſame manner $mD' = a + c \operatorname{ſin} \phi^2$. From theſe equations we obtain, rejecting always the higher powers of $c$,

$c = \frac{m(D' - D)}{2 \operatorname{coſ} \phi^2}$, $a = mD' - \frac{m(D' - D) \operatorname{ſin} \phi^2}{2 \operatorname{coſ} \phi^2}$; and

$\frac{c}{a} = \frac{D' - D}{2D' \operatorname{coſ} \phi^2}$.

Theſe formulas may be transformed into others a little more convenient for computation, by putting ſec $\phi^2$ inſtead of

$\frac{1}{\operatorname{cof} \phi^2}$, and tan $\phi^2$ inftead of $\frac{\operatorname{fin} \phi^2}{\operatorname{cof} \phi^2}$; we have then,

$$c = \frac{m}{2} (D' - D) \sec \phi^2,$$

$$a = mD' - \frac{m}{2} (D' - D) \tan \phi^2, \text{ and}$$

$$\frac{c}{a} = \frac{(D' - D)}{2D'} \sec \phi^2.$$

Example of the compreffion deduced from the meridian and its perpendicular.

19. We may apply thefe formulas to the computation of $\frac{c}{a}$, &c. from the degrees of the meridian and perpendicular, meafured in the fouth of England. We find, in one example, (*Phil. Tranf.* 1795, p. 537), that D = 60851 fathoms, D' = 61182, the latitude, or $\phi$ being = 50° 41'. From this $\frac{c}{a}$ = $\frac{D' - D}{2D' \operatorname{cof}^2 \phi} = \frac{331}{2 \times 61182 \times (\operatorname{cof} 50^\circ 41')^2} = \frac{1}{148.4}$, which is nearly the fame refult with that deduced in the paffage juft referred to. Indeed the folution of this problem, contained in the *Trigonometrical Survey*, is quite unexceptionable; and the theorems here offered are not given as containing a more accurate folution, but one that is in fome refpects more fimple.

Whether the magnitude of this refult may prove an irregular figure.

The above compreffion, if the remarks already made be well founded, is much too great, being more than double of what was obtained from comparing the whole arch of the meridian meafured in France with the whole of that meafured in Peru. At the fame time it is right to obferve, that all the other comparifons of the degrees of the meridian, with thofe of the curve perpendicular to it, made from the obfervations in the fouth of England, agree nearly in giving the fame oblatenefs to the terreftrial fpheroid. For this circumftance, it is certainly not eafy to account; the unparalleled accuracy with which the whole of the meafurement has been conducted, makes it in the higheft degree improbable that it arifes from any error; and even if errors were to be admitted, it is not likely that they fhould all fall on the fame fide. The authors of the *Trigonometrical Survey* feem willing, therefore, to give up the elliptic figure of the earth, (*Ibid.* p. 527); but before we abandon that very natural and fimple hypothefis, it may perhaps be worth while to attend to the following confiderations.

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20. In the part of England, where the meafures we are now treating of have been taken, the ftrata are of chalk, and though of great extent, are bordered, on all the fides that we have accefs to examine, by ftrata much denfer and more compact. Toward the weft the chalk is fucceeded by limeftone, and that limeftone by the primitive fchiftus and granite of the weft of Devonfhire and of Cornwall. On the eaft we may fuppofe that fomething of the fame kind takes place, though the fea prevents us from obferving it, as the chalky and argillaceous beds extend in this direction to the coaft, and probably to fome diftance beyond it. Now the meridian of Greenwich may be confidered as dividing the tract of country, occupied by thefe lighter ftrata, into two parts, in fuch a manner, that the plummet being carried to a diftance from it, either eaft or weft, approaches to the denfer ftrata, and is of courfe attracted by them, fo that the zenith is forced back, as it were, to the meridian of Greenwich, and does not recede from it, in the heavens, at fo great a rate as the plummet itfelf does, on the earth. Hence the longitudes from this meridian, eftimated by the arches in the heavens, intercepted between the zenith and the faid meridian, will appear lefs than they ought to do; and too much fpace on the furface of the earth will of confequence be affigned as the meafure of a degree. In this way $D'$ is made too great; and we may fuppofe the circumftances fuch that $D$, on going north or fouth, is not enlarged in the fame proportion; hence $\frac{D'-D}{D'}$ will be augmented, and of courfe $\frac{c}{a}$ will be reprefented as too great. This explanation may perhaps appear very hypothetical, and it is certainly propofed merely as a hypothefis. It is a hypothefis, too, that lays claim only to a temporary indulgence, as it is propofed at the very moment when it may be brought to the trial, and when, by a further continuation of the furvey toward the north, it will probably be determined how far the diftribution of the ftrata of this country affects the direction of gravity. It will indeed be curious to remark what irregularities take place on advancing into the denfer ftrata of the north. The limeftone and fandftone ftrata of the middle part of the ifland will fucceed to the chalk of the fouth, the primitive and denfer ftrata ftill

Obfervations to fhew that the irregularity probably arifes from local circumftances.

The meridian of Greenwich appears to divide a chalk country, bearing on each fide upon denfer ftrata of limeftone, fchiftus, and granite, which, by attracting the plumb-line either way outwards, muft require a longer meafure of eafting or wefting to correfpond with an apparent celeftial arc.

 occupying

occupying the weſt, at leaſt at intervals, as in Wales, Cumberland, and Galloway. Further to the north, that is, beyond the Tay, the ſtrata became entirely primitive, moſt of them of the denſeſt kind, and in the interior of the iſland, with a very few exeptions, continue the ſame to its moſt northern extremity. In the ſurvey of Britain, therefore, ſeveral ſituations muſt occur where the plummet, paſſing from lighter to denſer ſtrata, ought to give indications of ſome irregularities in the direction of the gravitating force. It will be ſeen hereafter how far theſe conjectures are verified by experience.

*(To be continued.)*

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## IV.

*On the Determination of the Length of the Solar Year. By Mr.* R. WINTER.

*Red Croſs Wharf, London-Bridge,*
*13th Jan.* 1804.

To Mr. NICHOLSON,

SIR,

Determination of the length of the ſolar and ſidual years.

SHOULD you conſider the following communication as deſerving a place in your valuable Journal, by inſerting it you will oblige

Your's, &c.
R. WINTER.

To determine the preciſe length of the Solar Year is an object of primary utility in aſtronomy, as being the baſis for aſcertaining the periods of all the moving bodies in the univerſe.

Various and accurate methods have been given for determining this period; but as the following unites ſimplicity, to the advanced ſtate of ſcience, theſe characters may render it worthy of your conſideration.

Given, the time and place of the ſun in the ecliptic, and the time of its returning to the ſame place; the obliquity of the ecliptic for the preſent time, together with the decreaſe thereof, per year, to find the length of the ſolar year.

According to Ptolemy, the place of the ſun was Sagittarius 21° 22′ 50″ anno. 130, Dec. 13th. $11^h$ $38^m$ $40^s$ (reduced to the

the meridian of London,) its return to that place in 1801 according to Mayers tables improved, is Dec. 13th. $0^{h.}$ $19^{m.}$ $36^{s}$ the interval, (allowing for biffextiles, &c.) is 611416$^{D.}$ $42^{h.}$ $40^{m.}$ $56^{s.}$ which divided by 1674, the number of years, gives $365^{D.}$ $5^{h.}$ $49^{m.}$ $42^{s.}$ Now the obliquity of the ecliptic, as obferved by Caffini in 1655, and Flamftead in 1689, being compared with that obferved by Hornfby in 1772, Mafkelyne in 1769, Bradley in 1750, and Mayer in 1756, and a mean of their obfervations taken, will give for the decreafe of the obliquity of the ecliptic 59″ per century, or 35′.″4 per year. According to thefe calculations the obliquity of the ecliptic for 1801 will be about 23° 27′49″; then as the fun moves over this fpace in one quarter of a year, fay as 23° 27′ 49″ is to ¼ of the given year, fo is the decreafe of the obliquity of the ecliptic per year 35‴4 unto 55″, which taken from $365^{D.}$ $5^{h.}$ $49^{m.}$ $42^{s}$ gives for the length of the folar year $365^{D.}$ $5^{h.}$ $48^{m.}$ $47^{s.}$ and the time the fun moves over the proceffion of the equinoxes is 20 minutes 27 feconds, which being added to the folar year will give $365^{D.}$ $6^{h.}$ $9^{m.}$ $14^{s.}$ for the length of the fidereal year.

But by affuming ¼ of the year thus found, and working as before, it will give the length of the year more accurately.

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## V.

*Extracts from a Letter of R. Chenevix, Efq. F. R. S. from Drefden, to Charles Hatchett, Efq. F. R. S. containing Information refpecting the new Metal contained in Crude Platina; Beet Sugar; the non-exiftence of Aguftile as a peculiar Earth; and a Defcription of a new Furnace for Chemical Operations. Communicated by* C. HATCHETT, *Efq.*

I SUPPOSE you have heard of the new metal contained in raw platina. Here is what Berthollet has juft written to me upon the fubject: New metal in crude platina.

"Des Coftils diffolves raw platina in nitro-muriatic acid, and precipitates it by muriate of ammonia at feveral times; the firft portions are yellow, the laft redder. He reduces the red precipitate, and obtains an alloy. He expofes this alloy to a current of oxygen, and a blue oxide is volatilized; pure platina remains behind. The blue fublimate is the oxide of his

new

new metal; on the ſame day Fourcroy and Vauquelin read their paper at the Inſtitute, and mentioned ſimilar experiments."

Palladium aſſerted to be an alloy of the new metal with mercury.

Van Mons writes me word that at Paris they ſay that it is not platina, but this new metal, which with mercury compoſes palladium; but that has very little to ſay to the purpoſe, for the ſingular thing in palladium is, not what regards platina, but what regards mercury, and the fuſibility of the combination. Beſides there muſt have been platina in my palladium, as I took the metal reduced from the red and yellow ſalts indiſcriminately.

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Manufacture of beet ſugar by Lampadius.

Lampadius tells me that he has made ſeveral hundred weight of ſugar from the red beet; but the manufactory he eſtabliſhed has failed. It coſt him within two gros per lb. as much as the common ſugar of equal quality, but then he had a reſiduum fit for fermentation, which was clear profit.

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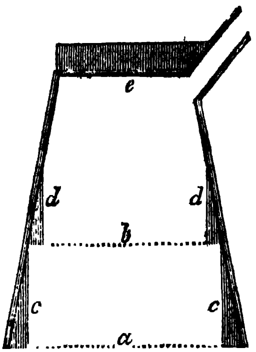
Aguſtite is merely phoſphate of lime.

I had made ſome progreſs in the analyſis of the Saxon beryl, when I received a letter from Berthollet, in which he told me that Vauquelin had found the aguſtite to be nothing more than phoſphate of lime. I continued my experiments however, and obtained the following reſults. Having boiled muriatic acid upon the pulverized maſs which contains the Saxon beryl, I obtained a ſolution of the pretended aguſtite. I precipitated by ammonia, and, upon examining the precipitate, found it to be phoſphate of lime. To have no doubt as to the phoſphoric acid, I treated 1500 grains of this precipitate with half its weight of ſulphuric acid, waſhed the maſs, and evaporated to dryneſs. I re-diſſolved as much as was ſoluble in diſtilled water, and ſaturated the liquor by ammonia. By evaporating once more, I obtained phoſphate of ammonia, which I decompoſed at a ſtrong red heat, and obtained phoſphoric acid, which gave no trace of ſulphuric or any other acid, or of any earth, alkali, or metal. I mixed this with pounded charcoal, and by diſtillation in a ſtrong fire obtained a conſiderable quantity of phoſphorus. Thus then the ſimple earth of Mons. Tromſdorf is phoſphate of lime.

Muriatic ſolution of Saxon beryl precip. by amm. gave phoſ. of lime. This was decompoſed by ſulph. acid; and the purified phoſphoric acid reduced to actual phoſphorus.

I have

I have conſtructed a wind furnace here, which is in ſome reſpects to be preferred to the uſual form. The ſides, inſtead of being perpendicular, are inverted, ſo that the hollow ſpace is pyramidical. At the bottom the opening is thirteen inches ſquare, and at the top but eight. The perpendicular height is 17 inches. This form appears to me to unite the following advantages. 1ſt. A great ſurface is expoſed to the air; which, having an eaſy entrance, ruſhes through the fuel with great rapidity. 2d. The inclined ſides act in ſome meaſure as reverberating ſurfaces: And 3d. The fuel falls of itſelf, and is always in cloſe contact with the crucible, placed near the grate. This I believe to be the principal advantage. The late Dr. Kennedy of Edinburgh, whoſe opinion on this ſubject claims the greateſt weight, found that the ſtrongeſt heat in our common wind furnaces was within two or three inches of the grate. That therefore is the moſt advantageous poſition for the crucible, and ſtill more ſo when we can keep it ſurrounded with fuel. It is inconvenient and dangerous for the crucible to ſtir the fire often to make the fuel fall, and the pyramidical form renders this unneceſſary. It is alſo more eaſy to avoid a ſudden bend in the chimney by the upper part of the furnace advancing as in this conſtruction. *a* is a grate; *c* and *c* are two bricks, which I can let in at pleaſure to diminiſh the capacity; *b* is another grate, which I can place upon the bricks *c* and *c* for ſmaller purpoſes; *d* and *d* are bricks which I can place upon the grate *b* to diminiſh the upper capacity, ſo that in fact I have four different ſizes in the ſame furnace. I have had ſome very ſtrong fires in uſing the whole capacity from the grate *a* to the top *e* without the bricks *c* and *c*; but I am at a loſs for our good Engliſh coaks. The bricks have all been ground down to the ſlope of the furnace and fit in with tolerable accuracy. They are totally independent of the pyramidical form, &c. of the furnace.




Chemical furnace. Fruſtum of a cone, narroweſt above. Its great advantage is that the fuel deſcends without the help of ſtirring.

## VI.

*Account of an Experiment to imitate the Damascus Sword Blades, In a Letter from Mr.* James Stodart.

To Mr. NICHOLSON,

Dear Sir,

On fword blades in general. Quality too often defective.

HAVING lately had an opportunity of examining fome fword blades, which appeared to be defective, I was induced to make the following experiment. The fubject is furely of fome importance, and perhaps never more fo than at the prefent moment. We hear of fwords having broken in battle, and we can hardly imagine a more diftreffing circumftance. Thofe which I have feen are certainly in no danger of failing in that way, for on the contrary they are evidently too foft, and confequently cannot form a good cutting edge. I am not acquainted with the procefs ufed in making fword blades, but am inclined to fufpect that the price allowed is not equal to the labour neceffary to form a good inftrument. The following method, which I believe to be nearly the fame as that practifed at Damafcus, but which I fufpect would be too difficult and expenfive for general application, may perhaps lead to fome more fimple method of accomplifhing the defired purpofe. I took fix fmall bars of good malleable iron, and the fame number of fheer fteel, and laid them one on another alternately, as if forming a galvanic pile; I then with the affiftance of an expert workman, committed them to a clean forge fire, and with care we fucceeded in welding them into a folid lump. This was forged into a ftout flat plate, which being heated to whitenefs, was by means of ftrong tongs twifted fpirally until it formed a cylindrical tube. In this twifted ftate it was heated, hammered flat, and again welded, and after being forged into a convenient form and fubftance, was doubled throughout its whole length, fomewhat in the manner of the back of a faw. A flip of good fteel was inferted, and another welding heat taken, which confolidated the whole mafs. I need not fay this flip of fteel was intended for our edge. The remaining part of the procefs was fimple; it confifted only in forging it into the fhape of the blade we wanted; which on examination proved perfectly found in every part. Being eager to witnefs fome proofs of excellence and beauty which my expectation had

Experiment. Steel and iron bars were welded together.

Forged out.

Twifted, flattened, again welded and hammered flat.

An edge of fteel was welded into a back of the compound.

had anticipated, I too haſtily and without due conſideration proceeded to harden it by heating and quenching in water; and had the misfortune to ſee it cracking in ſeven or eight different places. I have no doubt this was occaſioned by the unequal expanſion and ſubſequent contraction of the different parts of the maſs. In my next trial I ſhall guard againſt this accident. Enough however remained ſound to prove it both good and beautiful; the edge bears the ſevereſt trials at the ſame time that the whole blade has ſufficient tenacity. I have poliſhed a part of it, and by applying a weak acid, produced an appearance, which though by no means equal to the beauty of what is called the Damaſcus water, leaves me little reaſon to doubt of accompliſhing that appearance in my next trial. My intention is to multiply my pieces of metal, to repeat the proceſs of twiſting, and certainly not again to quench in water. I ſhall take the liberty to tranſmit to you an exact account of my next experiment, and if ſucceſsful, to accompany it with a ſample of the metal formed into a blade of ſome kind or another. I am with much reſpect,

It cracked in hardening.

—but promiſes well.

The Damaſcus water.

DEAR SIR,

Your obedient ſervant,

J. STODART.

*Strand, Jan.* 19*th*, 1804.

*P. S.* Why is the appearance produced on Damaſcus ſteel by the application of an acid called the water? Is it not different degrees of oxidation? * and what is the acid beſt fitted to produce this appearance. I had a paper given me ſome ten years ago on this ſubject, by a gentleman whoſe name I do not know. Unfortunately I have miſlaid it.

Inquiries concerning the Damaſcus water upon ſteel.

In addition to what you have publiſhed on the ſubject in your valuable Journal, pray furniſh us with any other facts that may have come to your knowledge ſince that period. The ſubject appears to me to be worthy of philoſophical reſearch, and perhaps of national encouragement.

* I have always ſuppoſed ſteel to be leſs readily ſoluble than pure iron; and that the carbon which is ſeen on the face of the former during the proceſs of damaſking, defends it from the acid, while the fibres of iron are etched by corroſion ſo as to exhibit the peculiar waving lines of this operation. N.

Letter

## VII.

*Letter from a Correspondent on the Effects of Thunder on fermenting Liquids; the Chemical Action of Sound, and Tremulous Motions, with other Observations.*

*Newcastle, Dec.* 16, 1803.

To Mr. NICHOLSON.

SIR,

CONVERSATION is certainly one of the most desirable means for the increase and dissemination of knowledge, whatever description it may be of, and if the following remarks, arising from an accidental conversation with a scientific friend, be not altogether unworthy of your notice, you may make what use of them you think proper; and if thought worthy of being presented to the public, you are at liberty to make any necessary alteration in the stile that will render them more worthy of the honour I solicit.

Changes produced in beer and in cream by thunder.

It has been observed, so often indeed, that it has almost become a popular remark, that the noise of thunder produces a surprising change on beer, and on cream: the first becoming sour, and the last rancid, when they are agitated by this extraordinary sound. It has also been observed, I believe, that the effects of thunder on malt liquors may be prevented by laying a cord over the cask, at each end of which, a stone, or other hard and ponderous substance is suspended and kept in contact with the outside of the vessel containing the liquid liable to be affected by this kind of noise.

Supposed preventative remedy.

The effect supposed to arise from tremulous action.

Now we may be able to form some judgment of the manner in which these liquids are affected, by attending to the tremulous motion produced on those glasses called singing and musical glasses, when the surface of the first is agitated by the breath; and the liquid in the last by the vibration produced by the finger drawn along the rim of the glass, which varies in its tone in proportion to the quantity of liquid it contains. In the course of the tone or sound, if the glass be pressed with the finger, the sound either ceases or is considerably diminished, and upon application of the finger at the commencement of the sound it will be evidently felt that both the glass and the liquid are in a state of agitation, and that this agitation ceases or stops when the agitated body is touched by another in a quiescent state.

This

This tremor, so communicated, may in my opinion be the commencement of that insensible or rather imperceptible agitation necessary to the production of the different states of fermentation, which when once began, may be continued, provided the circumstances of temperature, &c. be equally favourable as at first. Now we perceive that this motion is produced by sound;* and a sonorous body loses this vibration on being touched, and the sound itself of course ceases. In like manner the intestine motion arising from the vibration occasioned by the noise of thunder, being prevented from taking place by the contact of the suspended stones with the sides of the cask, the ensuing fermentation cannot commence, and the beer is preserved from running into the acetous fermentation.

Which may produce the commencement of the fermentative agitation.

This insensible motion is also necessary to assist the absorbtion of oxygen, without which neither beer can grow sour, nor can cream become rancid; for violent agitation will not produce the like effect even though accompanied or assisted with the necessary increase of temperature.

It was also observed by the gentleman with whom I was conversing on this subject, that butter by being over much washed or wrought in water loses considerably of its yellow colour, and acquires in a great measure the paleness and consistence of tallow. It might be worth while to make some experiments in this way, how far animal oils or fat are capable of decomposing atmospheric air or water. The circumstances necessary to determine the mutual change on each, are agitation, and a frequent change of surface. The absorption of oxygen would take place in both cases, the residuum of atmospheric air being azotic gas, and that of water hydrogen gas. I had found by experiments, and that some time before I saw the circumstance publicly noticed, I may say, even before the new theory of chemistry was promulgated, that the union between mercury and the fat with which it was combined in the composition of the ointment, became much more intimate and perfect on being kept for some time, and that the rancidity, which I thought arose from the developement of some peculiar acid, was prevented from becoming sensible by that acid being absorbed by the metal. From this circumstance, and from this supposition, I was led contrary to the directions and *commands*

Decomposition of water or air by oils.

Rancid oils combine better with mercury than fresh.

* I mean among other causes producing it.

of

of the Royal College of Phyficians, to ufe a little rancid fat or oil, in order to facilitate my labour and favour my indolence, and I was agreeably furprifed at my fuccefs. The College order the pureft and fweeteft fat to be ufed; neverthelefs the utmoft diligence and care in rubbing the mercury with it does not produce fuch an accurate combination at firft as is accomplifhed, after being fet by for a week or two, and this my be made evident by comparing two fpecimens of ointment made with equal care and diligence, the one being quite recently finifhed, and the other having afterwards been kept undifturbed for two or three weeks.—I merely offer thefe hints for the confideration of wifer heads than mine, and fhould they with this view be honoured with a place in your valuable repofitory, your indulgence will be moft refpectfully acknowledged by

Sir,

Yours, &c.

NORTHUMBRIENSIS.

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## VIII.

*Improvement of the Electrical Machine, chiefly with refpect to the Rubbers; by Mr.* Wolff, *of Hanover**.

Defcription of the machine.

The plate.

MY electrical apparatus is a plate machine, conftructed on the plan of that defcribed by Van-Marum †, with a few flight alterations, which are principally in the rubbers. The plate is eighteen inches in diameter, and the piece of wood, which receives the fcrew that faftens the plate to the axis, is an inch and half in diameter. The rubbers (at the extremities of which, toward the axis, are fticks of black fealing-wax, rounded at each end, and overhanging or projecting on three fides) are in contact with the glafs for the length of $5\frac{3}{8}$ inches. Confequently the diftance from the circular or central piece of wood to the rubbers is $2\frac{1}{8}$ inches, to which extent the plate

* From Gilbert's *Annalen der Phyfick*, 1802, No. 13. p. 601.

† *Obfervations fur la Phyfique*, Vol. 38. p. 437. or Philofophical Journal, 4to. Series, No. 2.

is coated on both ſides with a ſufficient thickneſs of varniſh *. The bow of the conductor, which conſiſts of a ball ſix inches in diameter, to which is added a moveable tube with ſeveral fitting pieces that terminate in balls of different ſizes, had at firſt, at each of its extremities, as in Van-Marum's machine, a braſs cylinder three inches long and one inch thick, with an hemiſphere at each end. But as conductors of this conſtruction are liable to diſcharge themſelves, on account of the ſmall diameter of their hemiſpheres, even before they have received their full charge, I have preferred putting one braſs ball three inches in diameter on the upper end of the ſuperior conductor; and another on the lower end of the inferior conductor. Theſe balls likewiſe ſerve the purpoſe of collecting the fluid excited at the extreme parts; for the balls, both above and below, project conſiderably beyond the thickneſs of the plate, and prevent the eſcape of the fluid as much as is poſſible, particularly in a machine leſs high in proportion than that of Van-Marum. The conductor is only 4½ inches diſtant from the table, conſequently only half the diſtance of Van-Marum's; yet the fluid does not eſcape, till the conductor is overcharged. The eſcape of the fluid is prevented likewiſe by an inſulating ſquare of glaſs 42 inches, made rough and coated with copal varniſh, which is placed on the table beneath the lower conductor, its three ſhort feet being let into the table. This ſquare of glaſs may be uſed on ſeveral occaſions as an inſulating ſtand.

The conductor deſcribed.

The eſcape of electricity to a glaſs plate placed on the table beneath the conductor.

The four rubbers are made of dry walnut wood ſoaked in amber varniſh, and are 5⅜ inches long, 1½ broad, and a little more than ¼ of an inch thick. The metallic plate that communicates with the leather covered with amalgam, is only an 1½ inch broad, and is fixed externally to the centre of the piece of wood. The rubbers are preſſed toward the glaſs by means of a ſpring. They are covered with a piece of thick woollen, upon which is a piece of fine neat's leather. After the leather is faſtened to the wood, it is wetted, and preſſed between

The rubbers deſcribed.

* This varniſh is compoſed of copal, two ounces; gum ſandarac, one ounce; white amber, one ounce; olibanum, half an ounce; powdered and diſſolved in one pound of alcohol by digeſtion in a glaſs matras. When the ſolution is perfectly cold, it is paſſed through the filter.

Amber varniſh for coating the plate.

two

two boards, where it is kept till it is again dry. Thus it is rendered very flat, and its edge very ſharp, and all its parts will apply to the ſurface of the glaſs. This piece of leather is covered with another a little broader, the rough ſurface of which is towards the glaſs, and its lower edge on the ſide towards which the plate moves; and its upper edge on the other ſide from which the plate moves, being likewiſe very ſharp. The piece of ſilk is applied with accuracy to this leather. Before it is faſtened on, it is heated, and beſmeared firſt with butter of cacao, then with a large quantity of Kienmayer's amalgam *; and after it is faſtened on, it is compreſſed in conjunction with the wood, or preſſed ſtrongly againſt the machine. The leather is next covered with amber varniſh, amalgam is ſpread over this, and after the varniſh is dry, it is ſmoothed with a burniſher. This is repeated ſeveral times. The whole being very dry, and the rubber being preſſed ſo as to touch the glaſs in all points, the leather coated with amalgam is covered with a piece of fine white paper, as long as the leather, and half an inch broader, ſo as to cover the ſeam that faſtens the ſilk to the leather, and the paper is faſtened to the wood above or below, accordingly as it is on the aſcending or deſcending ſide of the plate.

The rubber covered with fine white paper.

What led to this.

Dry paper is known to be capable of acquiring a high ſtate of electricity, which induced me to try this ſubſtance as an immediate rubber. The following are the advantages, that by my experiments, repeated and varied in a great number of ways, I have found paper employed as a rubber to poſſeſs over every other known ſubſtance.

Its advantages.

The glaſs not rendered dull,

1. The glaſs is not rendered dull by the friction, as happens at length, and by frequent uſing, when it is in immediate contact with the amalgam.

does not become ſtreaky,

2. By the immediate contact of the amalgam, the glaſs frequently contracts ſtreaks here and there, that occaſion a circulation of the fluid. This cannot take place in the conſtruction I propoſe.

and neither it nor the ſilk is ſoiled. Cleanneſs of the whole machine advantageous.

3. Neither the glaſs nor the ſilk can be ſoiled. It is well known, that the cleanneſs of the glaſs, as well as of the rubber and the whole machine in general, is of importance in producing

* I add to this amalgam as much ſilver, as the mercury can diſſolve in conjunction with the zinc.

producing an intenſe degree of electricity. It is true, that it has heen propoſed to apply the amalgam to the glaſs inſtead of the rubbers; but the greater effect, that ſeems to be produced by this laſt method, is only apparent, and conſiſts entirely in the circulation of the fluid on the glaſs, while far from exciting or accumulating more of the fluid, this proceſs and the circulation diſperſe it.

Rubbing the plate with amalgam inſtead of the rubbers a bad method.

4. The amalgam on the leather does not require to be frequently renewed. The duſt of the amalgam, that is depoſited on the edges of the paper, is injurious only when accumulated there in ſufficient quantity to be conveyed to the glaſs, from which however it may eaſily be removed.

The amalgam does not require frequent renewing.

5. The return and paſſage of ſparks to the rubbers are rendered more difficult, as the paper ſufficiently covers the borders of the rubbers, that are turned toward the axis.

The return and paſſage of ſparks to the rubbers more difficult.

6. In my conſtruction the rubbers may be larger than in the uſual way, and in reality they are larger in proportion in my machine than in Van-Marum's. No ſpark paſſes the axis, unleſs the air be very damp; for the fluid, in caſe of a ſtrong accumulation, flies in preference to the oppoſite rubber over a quarter of the periphery of the glaſs. I am perſuaded, that, by adopting my conſtruction, the rubbers of a plate of 32 inches, ſuch as Van-Marum's is, may be eleven inches inſtead of nine, in which caſe there would ſtill be two inches for the diameter of the piece of wood that faſtens the plate to the axis, and three inches for the diſtance from this piece to the rubbers; which I think would be ſufficient in theſe circumſtances; and the friction being on a larger ſurface of the plate, the effect muſt naturally be much greater. I ſhall try this alteration of the rubbers on large plates of Bohemian glaſs, as well as on Engliſh cylinders of 18 inches diameter, and 21 inches long. The reſult I have already obtained with a ſmall cylinder gives me reaſon to hope much more complete ſucceſs with a large one.

The rubbers may be longer.

Sparks do not paſs to the axis.

Farther trials intended.

7. With my rubbers the friction may be rendered much greater, than with thoſe the amalgam of which is in immediate contact with the glaſs, and ſoils it; beſides, the plate turns with an uniform friction.

The friction may be made much greater, and is uniform.

8. The activity of the machine is extraordinarily increaſed by this conſtruction. The greater freedom with which the plate

The activity of the machine extraordinarily increaſed, and why.

plate moves, even under a greater preſſure, and he paper's preventing the glaſs from being ſoiled, would be ſufficient to produce this effect; even if the greater preſſure alone did not occaſion a more powerful effect than can be obtained from common machines.

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## IX.

### *On the Cultivation of the Sunflower, and its Advantages* *.

The ſunflower recommended to cultivators.

THE ſunflower, *helianthus annuus L.* is ſaid to have been brought to Europe from Peru. There are two ſpecies, one annual, the other perennial: of theſe the latter is only ornamental, but the annual is of ſuch uſe in agriculture, that it deſerves to be made known to our readers.

Seeds,

have two varieties.

The ſeeds are white, gray, or blackiſh; but this difference of colour is entirely accidental, as ſeeds of either colour produce the others reciprocally. There are two varieties, however, that appear to me permanent, the ſunflower with a ſingle ſtem, and that with a branchy ſtem, which is leſs common.

Method of cultivation.

The ſeed ſhould be ſowed in ſpring, when nothing is to be apprehended from the froſt, that would deſtroy the young plants. The ground ſhould be well broken and manured, if you would have an abundant crop. You may ſow broadcaſt, and afterward thin the plants: but it is better to dibble the ſeed, placing two in a hole, the holes a foot diſtant, and the rows two feet aſunder. If both ſeeds germinate, the weaker plant ſhould be pulled up. The plants ſhould be weeded, and the earth dug up between the rows at a proper time. The height of the plant will be from ſix to nine feet: the ſtalks are large, ſome of them being ſeven or eight inches in circumference near the ground. It flowers in July or Auguſt, and the ſeeds are ripe in Autumn, at the ſame time with Indian corn. Rainy ſeaſons deſtroy many of the plants: the foot of the ſtalk rots in the ground, the leaves ſuddenly dry up, the ſtalk breaks of at the root, and the plant dies. A few ſunſhiny days ſtop the progreſs of this diſaſter.

Rainy ſeaſons bad.

* *La Decade Philoſophique*, No. 26. June, 1803. p. 507.

The

The leaves of the sunflower furnish abundance of agreeable fodder for cattle: they are gathered in succession without any perceptible injury to the plant; and after this crop of excellent fodder, you may expect another of seeds, that is very abundant. Some stalks afford not less than ten thousand. The best mode of gathering them is to cut the flower stalks, and, as the calyx is very thick, to hang them up in an airy place, that they may dry speedily. The leaves are excellent fodder. Mode of gathering the seed.

When they are in flower bees flock to them from all quarters to gather honey. Bees fond of the flowers.

The seed is rather farinaceous than oily, which they who have attempted to express oil from them did not consider. It is true, that oil may be extracted from them, but in quantities too small to make it worth while to cultivate the plant for this purpose. The seed affords oil, but in small quantities.

But if the seeds of the substance be incapable of affording oil with advantage, they are valuable for feeding animals commonly kept in the country. They are perfectly well suited to sheep, pigs, and other animals; but they produce a more striking effect on poultry. For these no food is more profitable, or occasions them to lay more eggss. Good food for sheep, pigs, &c. particularly for poultry; promoting their laying.

The dry stalks burn well, and afford very good ashes for lye, because they contain a great deal of alkali. In short, from the ease with which it is cultivated, the abundance of its produce, and the change it makes in crops, the sunflower may be considered as a new source of wealth to the farmer. The stalks burn well, and afford much alkali.

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## X.

## *Chemical Examination of a new Vegetable Salt, and of a new Acid discovered in it: by* KLAPROTH *.

THE vegetable production, which forms the subject of the present examination, affords a fresh proof, that hot climates have not only a great influence on the elaboration and particular modification of a great number of vegetable substances, but that they cause productions to be formed, the least vestige of which is not to be found in the same plants in the northern regions. Hot climates modify the vegetable products of cold, and create new ones.

* Scherer's *Allegemeines Journal der Chemie*, 1803: No. 55. p. 1.

A faline mafs exuded from the trunk of a mulberry tree in Sicily.

This production confifts of a faline mafs, excluded from the trunk of the white mulberry, *morus alba L.* which was obferved and gathered by Mr. Thompfon, in the botanical garden at Palermo.

External characters.

The faline mafs in its native falt, as found on the furface of the bark, has the appearance of a coating in little gramulous drops of a yellowifh and blackifh brown. The fubftance of the bark is equally penetrated with this falt.

Tafte.

The firft property by which it is diftinguifhed is its tafte, which comes neareft to that of the fuccinic acid.

Effect of heat.

On burning coals this falt fwells up flightly, emiting a vapour fcarcely perceptible to the eye, but irritating the organ of fmell, and leaves a flight earthy refiduum.

Lixiviation affords a light, pale falt, in radiating needles; not deliquefcent.

To feparate the falt, fix hundred grains of bark loaded with it were cut off, and lixiviated with a fufficient quantity of water. The lixivium, which was of a brownifh red colour was filtered, and evaporated for cryftallization; when three hundred and twenty grains of a light falt were obtained, refembling in colour a pale wood, and compofed of fhort needles united in radii, and not attracting the moifture of the air.

Little foluble in water, though foon cryftallizable.

Though thefe cryftals do not form till the lixivium is greatly condenfed by evaporation, the falt, neverthelefs, belongs to the clafs of thofe that are little foluble; for 1000 parts of water diffolve only 35 parts with heat, and 15 parts cold.

Contains no fulphuric acid.

The folution of this falt was not rendered turbid by the acetite or the water of barytes, which proves, that it contains no fulphuric acid.

Lime precipitated from it.

The alcalinule carbonates precipitate from it, an earth of a brown wood colour, which by a flight heat is calcined to whitenefs. This earth diffolves with effervefcence in nitric acid, and was precipitated from this folution in the form of fulphate of lime by means of fulphuric acid; and, in that of oxalate of lime by oxalic acid.

Precipitates acetite of lead, and nitrate of filver and mercury.

The folution of the falt readily precipitates acetite of lead, and the precipitate formed is reduced on burning coals. The nitrate of filver was precipitated by it in a pale brown, light, and fhining fcales, and the nitrate of mercury in whitifh flocks.

Hence it contains lime, with a peculiar vegetable acid, and extractive matter.

Thefe experiments indicate, that the falt in queftion is a neutral falt, compofed of lime and a peculiar vegetable acid. This acid, however, was combined with an extractive matter, which rendered the precipitates, that otherwife would have been white, either of a deep or pale wood colour.

Fifty

Fifty grains of the calcareous salt were heated to incandescence, in a retort communicating with the pneumatico-chemical mercurial apparatus, and twelve cubic inches of hidrogen gas mingled with carbonic gas were obtained; that burnt with a strong flame. The bulb of the neck of the retort contained an acid liquor, on which swam a fluid brown oil, both together weighing seven or eight grains. The residuum in the retort consisted of a spongy conglobulated mass of a pale brown colour, interspersed with coally particles, and dissolved in nitric acid with effervescence. This solution, being filtered, was precipitated with carbonate of ammonia; when 21 grains of carbonate of lime were separated. The coally matter that remained on the filter was very light, weighed four grains and half, and burned on a test, leaving a residuum of a quarter of a grain of calcareous earth.

By strong heat it gave hidrogen and carbonic gases,—an acid liquor, and a fluid brown oil, having a spungy residuum, containing lime, and a carbonaceous matter.

Another portion of the calcareous salt was precipitated by carbonate of ammonia, and the liquid remaining was evaporated by a gentle heat to crystallize it, when it furnished a salt in long and slender prisms.

By carbonate of ammonia, a salt in long slender prisms was obtained.

The liquor left, after the crystals were separated, was examined by several metallic re-agents. The solutions of silver, mercury, copper, iron, cobalt, and uranite in nitric acid, and those of lead and iron in acetic acid, were powerfully precipitated by it. The precipitate of copper was of the colour of verdigris; that of cobalt of a pale reddish colour; that of uranite of a yellowish colour; that of iron of a dull brown; and those of silver, mercury, and lead, of a bright brown wood colour.

The remaining liquor precipitated various metallic solutions.

The same liquor was rendered slightly turbid after a time by the water and acetite of barytes, the muriate of tin and gold, and the nitrate of nickel; but these precipitations might be the effect of the extractive matter, that adhered to the acid, rather than of a combination of this with the metallic oxides.

And the re-agents, perhaps owing to the extractive matter, rendered it slightly turbid.

I shall proceed to the experiments I made with a view of obtaining the acid of the calcareous salt pure.

Experiments to obtain the acid pure.

Forty-five grains of the precipitate obtained from the decomposition of the calcareous salt, by the acetite of lead, were mixed with a scruple of sulphuric acid diluted by a drachm of water. The sulphate of lead was separated, and the liquor evaporated. It afforded by crystallization thirty-four grains of acid salt in fine needles of the colour of pale wood.

The precipitate with acetite of lead mixed with sulphuric acid. Decomposed by sulphuric acid.

Decomposed by Sulphuric acid.

In the same manner thirty grains of the dry calcareous salt were decomposed by twelve grains of sulphuric acid, properly diluted. The sulphate of lime being separated, the same acid salt was obtained.

Has the taste of succinic acid; is not deliquescent, dissolves readily in alcohol and does not precipitate metallic solutions.

The taste of succinic acid is still more marked in the acid itself. The acid salt remains dry in the air; it is easily soluble in alcohol as well as in water. It does not precipitate the metallic solutions like its salt.

In the retort an acid liquor came over; prismatic, colourless, transparent crystals sublimed; and a coal remained.

Twenty grains of the acid salt were slightly heated in a small glass retort. First, a couple of drops of an acid liquor came over, the taste of which was perfectly analogous to that of the concrete acid. Next a concrete salt arose, that adhered flat against the top and part of the neck of the retort, in the form of prismatic crystals, colourless and transparent. A coally residuum remained in the retort.

The acid adheres strongly to the lime.

A similar saline sublimation was not observable with the calcareous salt: we must therefore conclude, that the acid adheres strongly to the lime, and cannot be separated from it by heat, without being decomposed.

The acid washed out and crystallized.

To separate the sublimed salt from the coally residuum, the whole contents of the retort were dissolved, and the liquor filtered. The solution was perfectly clear, and by spontaneous evaporation deposited the acid salt in colourless crystals.

Sublimation the best mode of obtaining it pure.

From this it appears, that a gentle sublimation is the best mode of obtaining the pure acid salt, and freeing it from the extractive matter, to which it adheres too strongly to be separated from it in the moist way.

Perhaps may be obtained from mulberry trees in this climate.

The small quantity of the calcareous salt I had left, did not allow me to carry the examination as far as I could have wished, in order to determine the specific characters of the acid. It is possible, that the mulberry trees in this climate may likewise furnish the same salt, and I shall proceed to inquire into this without delay.

A new acid, the *moroxylic*; the salt *moroxylate of lime*.

The experiments related, however, seem sufficiently to establish the point of the acid obtained, being a new vegetable acid, which comes nearest to the succinic, both in its taste and other qualities. We may therefore provisionally give it the name of *moroxylic acid*, and the calcareous salt containing it that of *moroxylate of lime*.

XI. On

## XI.

*On Mr.* WOOLF'S *Invention for equalizing the Action of a Crank, in contradistinction to the Effect of the Fly, in producing an uniform Effect against a variable Resistance. In a Letter from Mr.* T. JONES.

To Mr. NICHOLSON,

SIR,

I BEG leave to second Mr. Farcy's request for an explanation of the substitute for the fly and equalization of the action of the steam engine, in producing a rotative motion, given in your valuable work for November last: or, I shall esteem it a favour if you can direct me where I can see it at work *. It appears to me, that the inventor has lost sight of the value of that organ which he intends to supersede, and that he has not applied it in cases, where a machine is required to produce an uniform effect on a variable resistance, in which case the mere equalisation of the action of the power on a revolving crank would have no value at all.

Inquiry respecting Mr. Woolf's substitute for a fly.

The best attempt that I have seen to unite these two important points, is in a steam engine erected by Mr. Hornblower, at Messrs. Meux and Co's. brewery, where the action of the power on the pin of the crank, may have been in the ratio of the sines of each respective arc in its revolution round the arbor of the fly, I say, "*may have been*," and I wonder it was not, since that mode of communication would have so easily admitted it,—theoretically at least: then indeed, a very light fly would serve to turn the crank past the upper and nether part or points of the circle; but it could not have answered in regulating the unequal resistance which is found even in grinding malt, or pump works: and in my humble judgment, the fly must for ever keep its place and importance with every judicious mechanic.

Another attempt at the same effect.

I am, in much bodily indisposition,

SIR,

Your humble servant,

T. JONES.

*Surry Road, 7th January* 1804.

* If there be one at work near London, I have no doubt but Mr. Woolf the inventor, who is resident engineer at Messrs. Meux's brewery, will readily shew it. N.

Farther

## *Farther Remarks on Mr.* WOOLF's *Rotatory Apparatus.*

Farther account of Woolf's rotatory apparatus.

IN order to render the effect of Mr. Woolf's contrivance (*Fig.* 1. *Plate* XI. Vol. VI. November 1803.) more intelligible to my readers in general, I shall here explain the same in a popular way.

I will suppose, by way of simplifying the subject, that the rod B is so long as to act always nearly in the perpendicular, and that the action upon the small wheel is also in the same direction, namely, constantly that of gravity. The engraver has omitted to continue the rod of the piston supposed to work in the barrel G, and the proof came too late for it to be altered in time. The effect at the pin F in the little wheel, is precisely the same as if a weight was continually hanging there.

Effect of the power and weight in different positions.

1. In the position here shewn, the power of the engine does not act at all, and the weight at F has its full operation. —2. When C has moved through half the quarter circle to the right hand, F will have arrived at the lowest point; consequently the power of the engine will act by a lever equal to the sine of 45°, or seven-tenths of the radius: or seven-tenths of the full power of the engine will then act on the great wheel, and the weight at F will not act at all: —3. When C has moved through an entire quadrant, the power of the engine will be the greatest possible on the great wheel, but at the same time the weight will have arrived at the horizontal line beneath E, and will oppose the former action. The motive force will therefore be equal to the excess of the power of the engine beyond the weight. 4. When C has moved through another half quadrant, the action on the great wheel will be equal to seven-tenths, as in No. 2. and the weight, having arrived at the highest point will not act at all. 5. When C has moved through half a circle, and is at the lower station in the great wheel the engine will not act, and the weight having arrived again at F. will have its full operation as in No. 1.

There are four positions in which the counter-weight has no effect;

By pursuing this simple method of examination, we see that 1. there are four positions in which the arm C standing at an inclination of 45° to the horizon, and the arm F perpendicular to the same, the regulating weight can have no effect,

effect, whatever be its meafure; the action will be effectual to feven-tenths of the direct or entire action of the arm B in the tangent to the great circle; 2. that when the arm C is either perpendicularly up or down, the regulating weight alone acts in the fame tangent; 3. that when C lies horizontal, the effective force will be the excefs of the power of the engine beyond that of the weight. Since therefore we can do nothing with regard to the four firft mentioned pofitions, we have only to confider the four laft, and to contrive that the moving force fhall be the fame at the top and bottom, as at the two fides; or in other words, that the weight alone fhall be equal to the excefs of the power of the engine beyond that weight; which is the fame thing as faying, that the weight muft be equal to half the power of the engine.

two in which the weight acts by itfelf; and two in which the effect is produced by the excefs of the power beyond the weight.

The weight muft be half the power.

We fee then that the action on the wheel C, will vary from feven-tenths to five-tenths, four times in every revolution; the extreme difference being therefore two-tenths each way from the medium, which is about fix-tenths. It may not perhaps be of any practical value to difcufs the gradation by which the increafe and diminution of the oppofite actions are governed; and the mathematical reader will eafily fee that the direct action of each is as the cofine of the angle of the obliquity of the radius it acts upon. Still lefs does it feem to be of any confequence in the prefent loofe view of the fubject, to treat of any affumed obliquity of the rods of communication.

Variation one tenth on each fide the medium action.

If we fuppofe a power to act conftantly and uniformly, but with alternations of oppofite direction, in equal times and through like fpaces; and wifh to produce, by that action, a rotatory motion, we might in theory obtain the effect by a double fpiral, known by workmen by the name of a fnail: but the difficulties from friction, fhake, and other impediments, would in many inftances render the conftruction ineligible. The crank is the moft cheap, eafy, light, and practicable means of producing rotation from alternate right-lined motion and the contrary; and its properties are too well known to require difcuffion in this place. Its inequalities demand a fly, whether the power or the refiftance be uniform or variable; and I do not fee any ftriking difference between Mr. Woolf's contrivance

Concerning the equalization of actions, &c.

Snail.

Crank.

trivance and the fly, as far as concerns one fingle revolution, whether they be confidered as regulating any one of thefe. In both, the furplus power is employed in putting a weight into a fituation or ftate by which it is enabled to give back (in theory) the fame furplus, when the refiftance becomes greater or the power lefs. But if the whole power, in feveral fucceffive revolutions, fhould exceed the refiftance, and produce acceleration, the fly will continue to accumulate momentum, and will be ready to give it out, againft an augmented refiftance in fucceeding revolutions; fo that its equalizing power extends not only through each fingle revolution, but through many. I do not perceive this property in the ingenious contrivance before us.

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## XII.

*Obfervations on the extinct Volcanoes in the Environs of Coblentz, by the Chevalier* DE SADE. *Tranflated from the original Manufcript, communicated by the Count de* BOURNON.

Coblentz built of volcanic ftone, from the neighbourhood of Andernach. The Roman legions faid to have been driven from their camp near Andernach, by a volcano. This queftionable. The extinct volcanoes, are like thofe of Auvergne, of unknown date.

WHEN I refided at Coblentz in 1791, the volcanic ftones made ufe of in that city, led me to inquire whence they were procured. I was foon informed, that they came from the neighbourhood of Andernach; and the clergyman of that place acquainted me, that the extinct volcanoes were near Haach, an abbey of Benedictine monks, three leagues diftant. This worthy gentleman added, that he had fomewhere feen it mentioned, that the Roman legions, encamped near Andernach, were obliged to quit their poft, on account of the fire iffuing from the volcano. Its great diftance from the fea, made me doubt the truth of this; and my own obfervations have convinced me, that the extinct volcanoes of this country are at length as ancient as thofe of Auvergne, the date of which is unknown.

Beyond Andernach, river fediment intermixed with pumice ftones and volcanic fands. Agemberg, a fmall mountain on the weft,

Having procured a guide, and directing my fteps toward the fouth, I had fcarcely quitted Andernach, before I met with river fediment, intermingled with pumice ftones and volcanic fands. Leaving the high road, I proceeded toward a fmall mountain on the weft, called Agemberg. To

To my great satisfaction, after having traversed pumice stones and volcanic scoriæ, I arrived at the foot of a quarry of lava, from which hewn stones for building and millstones were taken. This quarry was wrought in the open air, and was covered with a bed of mould about eighteen feet deep. The thickness of this stratum demonstrates the high antiquity of the volcano, while at the same time it prevented me from tracing the course of the lava, particularly as above this lava, nothing is to be found but scattered blocks and fragments of lava, scoriæ, and pumice stones. I went to the top of the mountain, however, in hopes of discovering where the center might have been; for the mountain of Agemberg, all the rocks of which are torrefied, and the state of scoriæ, was not a crater. The mountains, by which it is surrounded on the west, appeared to me to be of the same nature, without any indication of the mouth from which the fire issued.

A quarry of lava, from which hewn stones and millstones were procured. Wrought in the open air, and covered with eighteen feet of mould. Above it scattered blocks and fragments of lava, scoriæ, and pumice stone. Agemberg not a crater, and the mountains, surrounding it, of the same nature.

The summit of mount Agemberg is very bare, but on descending a few paces, I found a very pleasant copse. A narrow path, which I took at a venture, led me, after some twenty steps, to a grotto, in which was a tomb, with an inscription from Job, reminding man of his nothingness. By the side of this grotto is another, inhabited by a hermit, with a table of volcanic stone in the centre, which he uses for different purposes. A little higher up, I discovered a third, smaller than either of these, containing a rustic altar; on one side was a pigeon-house, and lower down, two or three little basons of water bordered with shells.

The top of Agemberg bare. A little way down a pleasant copse, in which is a grotto, containing a tomb, another inhabited by a hermit, a third in which is an altar, a pigeon-house, and two or three basons of water.

I quitted this mountain with regret, to traverse another, which, though covered with wood, seemed to indicate the existence of a crater; but I could perceive no trace of one, the trees, and the thickness of the mould, letting me see nothing but scattered blocks of lava and scoriæ.

Another mountain covered with trees and deep mould, with scattered blocks of lava and scoriæ.

My examination was soon finished, and I resumed the road to the abbey of Haach. In this I found nothing but pumice stones, lava; in some places of the basaltic kind, scoriæ, a few fragments of hornstone schistus, and blocks of quartz; all scattered about on a stratum of earth, which itself was composed merely of river-sand and pumice stone.

In the road to the abbey of Haach, pumice stones, lava, in some places basaltic, scoriæ, hornstone, schistus, and blocks of quartz, on a stratum of river sand and pumice stone.

The

The crater said to be a lake near. Its situation fine.

The incumbent of the village belonging to the abbey, informed me, that the crater, of which I was in search, was a lake near his parish.

On the west side first argil of different colours, then hornstone, next scattered blocks of lava.

The situation of this lake displays much grandeur. It is surrounded by a chain of mountains covered with trees; its shape is oval, longest from north to south; and it requires near two hours to walk round it.

The bank a poor meadow.

The west side offers to view at first nothing but a spacious bed of argil, divided into bands of differents colours; after this appears a smaller bed of hornstone; and the rest exhibits only some scattered blocks of lava. The bank is an indifferent meadow.

On the north, blocks of lava, then hornstone cleft perpendicularly, and lastly, a fat, white clay, interspersed with blocks of lava and hornstone.

The north side begins with some of the blocks of lava just mentioned: next appears a pretty extensive bed of hornstone, the very numerous fissures of which are uniformly perpendicular to the horizon: and a little farther on, is a considerable bed of fat clay, as white as snow, interspersed with blocks of lava and hornstone.

On the east, compact and porous lava, and the mountain nearly perpendicular.

On approaching the east shore, nothing is to be found but lava, in some places compact, in others porous, and the mountain there is nearly perpendicular.

On the south a level meadow, and a shore covered with shells.

The prevailing winds N. and N. W.

Lastly, on the south side, the ground becomes level, and is simply a meadow. The strand is covered with shells, partly whole, partly broken; and hence I conclude, that the prevailing winds in this district, are the north and north-west.

The lake has no apparent influx of water, but a rivulet runs from it on the south.

Varies greatly in depth, more than 400 feet near the middle.

Abounds with fish, but the bottom too rocky to use nets.

This lake does not appear to me to receive water in any part: yet, on the south it has an outlet, that furnishes a small brook, watering the grounds of Medermich. The depth of the lake varies greatly, being upwards of four hundred feet near the middle. It abounds with fish, particularly pike; but the blocks of stone scattered over its bottom, render the use of the net nearly impracticable, so that the line is almost exclusively employed for catching them.

The abbey built by the count Palatine in 1093, he being alarmed by the nocturnal appearance of fires on the mountains.

The abbey, to which this lake belonged, was a spacious building, proclaiming the opulence of its founder. It was built in 1093, by Henry count of the Palatinate of the Rhine, and lord of this lake. Broverus, in his annals of Treves, says, that the phantoms and lights, which this prince saw every night on the mountains around him, induced him, to erect this pious foundation.

I was told, that the outlet I have juſt mentioned was artificial, and made fifty years after the building of the abbey. The foundations of this abbey, having been ſunk as deep as the outer walls are high above ground, had obſtructed the natural outlet, which occaſioned the water to riſe ſo high, as to inundate the convent, and render it neceſſary to form the preſent channel, in order that it might run off.

The preſent outlet artificial, the natural one having been obſtructed by the foundations of the abbey,

The face of the ground, however, makes me doubt, whether the lake ever flowed over on the ſide next the abbey; for to a conſiderable diſtance it riſes there uniformly as you proceed from the ſhore. It appeared to me, that the natural outlet muſt have been rather on the ſide toward Andernach, through fiſſures on the mountain. Neither can I believe, that this lake, if we may judge from its banks, was the crater of a volcano; but one of the great excavations produced by ſome earthquakes. In fact, I have already ſaid, that the eaſt and north-eaſt parts of this lake were volcanic: if therefore we ſuppoſe, that the ſummit of the mountain had been a volcano, its baſe muſt neceſſarily have become hollow, and in this ſtate of things, a very ſlight ſhock would have ſufficed to break the arch of the vault, or the cruſt that covered its abyſſes.

This queſtionable, as its natural outlet was more probably toward Andernach, through fiſſures in the mountain. Neither was it a crater, but probably produced by an earthquake.

We need not wonder, that this lake is always full of water, though it appears to receive none in any part; ſince it ſerves as a drain to the ſurrounding mountains, which are in great meaſure volcanic. Its water is very limpid, and it does not freeze except in very ſevere froſts. Marquart, in his origin of the Palatinate, aſſerts, that it was once larger than it is now; which in fact is perceptible on the ſouth and weſt, where the ground has the appearance of having been once covered with water. He adds, that precious ſtones and ſapphires were found on its borders. I did nor ſee any; but it is known, that there is a fact of this kind in the little rivulet of Pezzouliou, that runs among lava at Expilly, near Puy, in Velay.

Receives water from the ſurrounding mountains. Very clear, and does not eaſily freeze. Formerly larger than it is now. Sapphires and other precious ſtones, once found on its borders. A ſimilar occurrence at Expilly.

By no means ſatisfied with my reſearches, the principal object of which was, to diſcover the crater of the volcano, that could have produced all the lava in this country, I repaired to Medermenich, half a league to the ſouth-eaſt. There I found myſelf in quarries of volcanic ſtone, from which were taken mill-ſtones and hewn-ſtones for building, for

At Medermenick, quarries of volcanic ſtones.

for which there is a very great demand. These quarries are underground, and in these are shafts a hundred feet deep, to facilitate the working.

Depth of the strata.

The first stratum was mould, about 15 feet thick.
The second, scoriæ of lava, about 12

Total 27.

Descent into them.

The roof supported by walls and timber.

Beneath was the pure and compact lava. The descent into these quarries is by a stair of 120 steps, and a gentle descent of near 90 paces. The fragments of the stones got out served to make internal walls of support, to which were added stanchions of timber, in some places perpendicular, in others oblique, but always contrived to support the blocks separated by fissures. The stones were broken off by iron wedges, for they were afraid to separate them by blasting.

The stones separated by iron wedges.

The shafts numerous, and very dangerous from being left open.

These quarries wrought for several centuries.

I counted eighteen or twenty shafts in the space I traversed; and their mouths being perfectly open, woe to the traveller who should lose his way among them by night. I observed several of them covered up, indicating works abandoned; whence we may presume, that these quarries had been wrought for several centuries. Some old oaks likewise growing in the shafts thus covered confirm this opinion.

Its crater probably at mount Blemberg.

Beyond Medermenich a bed of the same lava, with a perpendicular face, forming a ravine 25 feet deep, and covered with a thin stratum of mould.

This top wrought, and the stone harder from its exposure to the air.

The thickness of the strata covering this lava prevented me from following its course, and tracing it to its source; but I judged, that the mountain of Blemberg, a league to the south-west, might be its crater. Accordingly I proceeded to examine it. A quarter of a league from Medermenich I found a bed of the same lava, the face of which was perpendicular, and forming a gully, that might be twenty-five feet deep. It is covered by a very thin stratum of mould, and is wrought into millstones, and stones for building. Being exposed to the open air, it is harder than that of Medermenich.

Beyond this, and some lava of the piperino species, is a cold acidulous chalybeate spring.

More lava in which were formerly quarries.

After having passed this gully, and a little bed of muddy lava, of the nature of piperino, we find in a meadow a very copious mineral spring. It is cold, and contains carbonic acid and iron. At some distance from this we meet with beds of lava, in which there had anciently been several quarries, and at length we arrive at the mountains of Blemberg. The surface of this mountain, though covered with trees on the east, is nevertheless full of torrefied and scorified lava, and gray and red pouzzolona, which left me no doubt, that this mountain had formerly been a volcano. The inspection of its summit

confirmed me in this opinion. Though divided into feveral parts, which do not perfectly correfpond with each other, that on the weft perfectly reprefented a fegment of a vaft crater in good prefervation. Standing on the border of this fegment, I obferved on the eaft, at the diftance of half a league, quarries of lava, of which this mountain was the fource. The gully I have mentioned feemed to militate againft the adoption of fuch an opinion with refpect to the quarries of Medermenich, which are to the north-eaft beyond thefe: but time, and the various circumftances, that have formed fifteen feet depth of mould, may have occafioned the apparent interruption of this current of lava. I do not doubt however, but feveral of the neighbouring mountains have been openings of volcanoes.

Blemberg covered with trees on the eaft; but abounds with lava and pouzzalana.

Its fummit once a vaft crater, but partly deftroyed.

Quarries of lava that proceeded from it.

As probably did thofe of Medermenich.

But feveral of the neighbouring mountains were once volcanoes.

From the fummit of Blemberg I enjoyed a grand and very extenfive profpect. On the fouth I had the little town of Mayerne, on the north Medermenich, and Ettringen on the weft.

Profpect from Blemberg fine.

I quitted this mountain to vifit that of Zimmeray, about half a league weft of it, and I paffed through the village of Ettringen, built of lava and fcoriæ.

Village of Ettringen built of lava and fcoriæ.

The whole of the body of the mountain of Zimmeray is volcanic. The lavas there are in a ftate of torrefaction, that announces the exiftence of a crater. In fact, when I reached its fummit, I there found all the marks of one. It is of a conical form, and has a hollow opening to the fouth. The fides of this hollow facing the fouth are well preferved: thofe that face the north have been deftroyed. I imagine this took place at the period of the laft eruption, and that a ftream of lava would be found by digging on that fide.

The mountain of Zimmeray volcanic.

Its fummit a crater.

I then left this mountain, and proceeded to that of Calberg, which is on the north, and feparated from it only by a tolerably broad valley, in which I found trapp, of a blackifh gray colour, in mafs, and in fcattered blocks. At the foot of mount Calberg was a bed of black, coarfe grained, volcanic fand, ftratiform, and containing a great deal of mica in large leaves. The trees that covered its fummit prevented me from afcertaining its nature, but the circumference of its bafe is entirely volcanic. Around it is muddy lava, in which larger or fmaller fragments of compact lava occur. Gray clay predominates there.

Mount Calberg contains trapp.

At its foot a bed of volcanic fand, containing mica in large leaves.

Its bafe entirely volcanic.

Gray clay predominates.

Once a volcano.

I had no doubt, that this mountain was once a volcano, its figure alone proves it. I went beyond it to the quarries that were working, and which were to the north, at the foot of a gentle declivity, separated from the Calberg by a valley, through which a small brook flowed to the east. These quarries are parts of vast beds of that kind of lava, which the Italians call piperino. It is in general an earthy lava, containing a pretty considerable quantity of fragments of compact lava. This stone is soft, and easily wrought. It is almost every where in a horizontal stratum, but varies in its texture, for in some places it appears in the form of a volcanic breccia, in others it has the texture of sand-stone.

Near it quarries of lava. of the piperino species; Soft and easily wrought; in some places a volcanic breccia, in others of a sandstone texture.

Having accomplished my object, which was to discover the volcanic openings, that furnished the lava of this district, I returned to the Medermenich, where I slept; and the next day I reached Coblentz. The plain I crossed was covered with fragments of quartz, trapp, and pumice stone. Near Octundung, on the high road, we find rocks of slate, which are working. After leaving this village four gently sloping mountains appear on the left, stretching from east to west, and separated only by narrow valleys. Their figure induced me to visit them; and I found them composed of pumice stones and scoriæ. The first and second are merely segments of two small craters, the northern part of which has been carried away. The other two, which are covered with trees, appear to me to have formed but one crater of vast extent, the southern part of which is destroyed. The lava of these little mountains is encrusted with an immense number of little crystals of piroxen. This reddish lava contains a pretty considerable quantity of reddish mica in large leaves, frequently half a line thick. Almost at the foot of the easternmost mountain this stone is procured, to be used in constructing walls, though it is too porous, and at the same time too hard, to be employed as hewn stone.

Road from Medermenich to Coblentz. The plain covered with fragments of quartz, trapp, and pumice stone. Near Octundung slate quarries. Near it four volcanic mountains. Their lava encrusted with small crystals of piroxen. It is reddish, and contains reddish mica in large leaves. Used in walls.

From the summit of these heights the prospect is beautiful and grand. From the east to the north appears the magnificent basin watered by the Rhine from the castle of Ehrenbreitstein to Andernach. The seven mountains (Siebenberge) raise their ancient heads on the north. On the west, the view includes from north to south all the mountains just described; and

View from these heights.

and on the south it is terminated by mountains covered with wood, the nearest of which appeared to me to be volcanic.

English gardens at Basnheim. Pumice stone on the road and in strata.

At length I arrived at Basnheim, remarkable for nothing but the English gardens belonging to the lord of the manor; and thence to Coblentz. I found pumice stone on the road, and in strata where pits had been dug in the ground.

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## XIII.

*On an Improvement in the Form of Spectacle-Glasses, by* WILLIAM HYDE WOLLASTON, *M. D. F. R. S. Communicated by the Author.*

Imperfection of spectacles, that objects out of center are distorted.

IT must have been remarked by persons who make use of spectacles, especially those who require glasses of short focal distance, that objects seen through them appear distinct, only when viewed through the central parts of the glasses; that when the direction of the sight E O, *Fig.* 1. page 145 is considerably inclined to the surfaces, objects appear distorted, and that this defect is greater in proportion to the greater obliquity of that line.

Remedy formerly proposed; to make the glasses smaller;— but to little effect.

It is on this account that opticians have lately made, and recommended spectacle-glasses of less diameter than those formerly in use, thinking that the extreme parts of the field of vision, which from indistinctness were of little use, might be spared without much inconvenience. But this alteration in the size of the glasses could hardly claim the merit of an improvement; since for one defect, it only substituted another scarcely less objectionable.

It seems indeed rather extraordinary, that, during five centuries which have elapsed since the invention of spectacles, neither theory nor accident should have produced any considerable variation from the original construction.

Huyghens proposed that the radii of curvature in spectacle glasses should be unequal.

It was indeed conceived by Huygens, that the glasses, instead of being equally curved on both sides, as is customary, should have the curvatures of their opposite surfaces in the proportion of six to one, because he had demonstrated that such a form was best suited to the object glasses of telescopes.

Repeated by Smith.

Dr. Smith also in his treatise on optics, (p. 258.) repeats this opinion of Huygens in the following cursory manner;

and

"and consequently this figure of a glass is the best for spectacles, as the double concave of like figure is the best to help short-sighted persons."

This is good for simple object lenses, but not for spectacles.

But although it may be very true that such a form of glass was best calculated for the object glass of a telescope, previous to the celebrated discovery of the achromatic object-glass by the late by Mr. Dollond, yet whatever advantages might at any time be expected from the telescopic object-glass so shaped, these were not to be obtained by a similar construction in spectacles, as may easily be seen by considering the different uses of the respective instruments.

Telescopic vision is nearly in the axis:—

In a telescope in the first place, our view is necessarily confined to a very small distance on each side of the axis; and secondly, every part of the object glass contributes to the distinctness of any object viewed.

and the whole lens acts for every radiant point.

It is under these circumstances alone that the proportion of the curvatures above-mentioned, might be proper for a single object glass, as being capable of collecting into the focus the rays that fall on every part of it parallel to the axis.

In spectacles it is very different.

By spectacles on the contrary, objects are to be viewed if possible in every direction in which they might be seen by the naked eye, which is often far removed from the centers of the glasses; consequently a constitution that is calculated to represent correctly central objects alone, cannot be the most advantageous.

A small part of the lens only is employed at once.

In these also, the portion of the glass employed at once is scarcely larger than the pupil of the eye, so that any endeavour to procure the concurrence of all parts of a glass in any one effect is evidently superfluous, and may also be shewn to be prejudicial.

Whence improvement must be sought from different considerations.

It is therefore proposed to remedy the imperfections observable in the spectacle glasses hitherto generally used, upon a principle suggested by this latter consideration, which presents an opportunity by a different construction of rendering objects in all directions distinct.

If the lens be made concave next the eye all the rays may be had to pass, through nearly perpendicular.

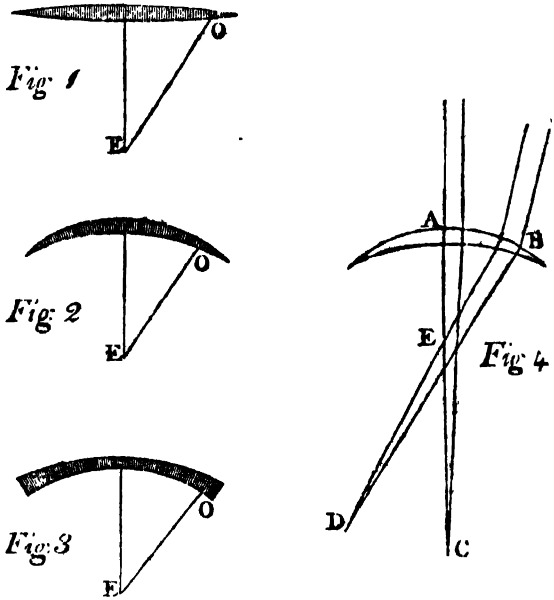
The alteration requisite for this purpose, is extremely simple, and easily intelligible. Supposing an eye to be placed in the center of any hollow globe of glass, it is plain that objects would then be seen *perpendicularly* through its surface in every direction. Consequently the more nearly any

spectacle-

fpectacle-glafs can be made to furround the eye in the manner of a globular furface, the more nearly will every part of it be *at right angles* to the line of fight; the more uniform will be the power of its different parts, and the more compleatly will the indiftinctnefs of lateral objects be avoided *.

**Convex outfide and concave within.**

According to this principle, all fpectacle-glaffes fhould be convex on their exterior furface, and concave within. The fection of thofe for long fighted perfons, will affume the form of a menifcus or crefcent, *Fig*. 2. and thofe adapted for fhort fight will have their principal curvature on the concave fide, *Fig*. 3.




**Experience confirms this doctrine.**

It is only neceffary to add, that the advantage of this improvement in the form of fpectacle glaffes has been confirmed by a fufficient number of experiments on different perfons, and that thofe in particular who are very long or very fhort fighted, are much benefited by them.

* To mathematicians it will be evident that any ray which does not pafs through the center of a lens, cannot be at right angles to both furfaces; but they will alfo perceive that when any fmall oblique pencil makes equal angles with the two furfaces of a thin lens, the inclination of it to each is fo fmall, that its focal length B D, *Fig*. 4. will not fenfibly differ from A C that of a central pencil.

 The

and a manufactory is eftablifhed.

The moft advantageous proportions of curvature for obtaining the different focal lengths now generally diftinguifhed by certain numbers, have alfo been duly confidered; and the manufacture of fpectacles, on this conftruction, has been undertaken by Meffrs. P. and J. Dollond, to whom the exclufive fale of them is fecured by patent, and whofe well known fkill in the conftruction of optical inftruments, enfures to this improvement every advantage of correct execution.

The fpectacles are called perifcopic.

From the opportunity afforded by thefe glaffes of *looking round* at various objects in one pofition of the head, it is thought they may not improperly be diftinguifhed by the name of *Perifcopic Spectacles.*

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## XIV.

*Letter of Enquiry concerning a Gun to throw double-headed Shot; with the Editor's Reply.*

To Mr. NICHOLSON.

SIR,

Drawing and defcription of a gun on a paper at the Royal Inftitution.

HAVING this inftant feen, at the Royal Inftitution, a fheet of paper printed by Nichols, printer, Earls Court, Newport Street, Soho, containing a drawing and defcription of a gun to throw double-headed fhot, I take the liberty of writing this on a page of a little note-book there, which muft be my apology for the paper, to entreat that you will obferve upon it in your invaluable Journal, becaufe I fear that it is not a fafe inftrument. A confiderable diftance is to be obferved between the powder fired and the fhot to be difcharged by it. I have always confidered that an interval fo kept was the occafion of the burfting of fo many guns. Pray is this fafe? And fhould not a line be cut on the arms to which the bullets are attached, to mark exactly the diftance of the attaching bar at each extremity from the muzzle, when the balls are equally rammed down. As the knowledge of its fafety may contribute to its fervice if really ufeful, I hope you will think this application not idle.

I am, Sir,

Your conftant READER.

ANSWER.

## ANSWER. W. N.

THE paper alluded to in the letter of my correspondent, contains an engraving of the shaded section of a gun having a double bore, but only one chamber; from which each bore directly leads: but they diverge from each other in an angle of about eighteen degrees, The charge of powder is to be placed in the chamber, and the two shot, one for each bore, are connected by a bar having two joints or hinges at an equal distance from its middle. By this contrivance, by bending the compound piece at the hinges, the shot can be put into the bores of the gun; and when the explosion blows them out, their divergence causes the bar to become strait, and consequently is expected to do mischief proportioned to the extent of its length. More particular account.

The title of the paper is, "Horizontal Section of a Gun for projecting double-headed Shot, invented by Lieut. Colonel Blaquiere, 22 Dragoons, July, 1803. Recommended to be used against an invading Army on board his Majesty's Gun-Boats;" and in the directions care is prescribed, that the same length of bar shall be left out of each muzzle to prevent accident; and it is stated to be a great advantage, that a considerable distance should be left between the powder and shot, as by this means the powder acquires its greatest possible force. Title of the paper and instructions.

My opinion was asked concerning a gun of this description which another inventor had proposed, some time previous to Midsummer last, to throw chain-shot; and the observations that then occurred were, 1. If there be the least difference between the windages or the blast in each bore, or in the casual impediments, one shot will precede the other. 2. If they go out fairly together, their velocity of separation will be to the direct velocity as the chord of the angle of divergence is to the radius. In the present gun the balls will separate with one-third of the velocity with which they advance. 3. That the violent separation of the shot from each other, will most probably, in either of these cases, break the chain, or the bar, or the joints. 4. Or if no fracture takes place, the sudden check will, by the elasticity of the metal, cause the shot to approach again to each other. 5. To which I may add, that it may be doubted whether the embarrassment of one of these bar-shots preceding the other might not occasion a dangerous obstruc- This gun invented also by another person. Its probable effects.

 tion,

tion, and that there are many facts which shew, that it is unsafe to permit any considerable expansion of, or space for, the blast of powder, before it shall act on the projectile.

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## XV.

*Prognostics of the Weather, established by long-continued Observation upon the Conduct and Appearances of Birds, Beasts, Insects, Plants, Meteors, the Heavenly Bodies, Minerals, &c. Communicated by a Correspondent.*

To Mr. NICHOLSON,

SIR,

Introduction. Prognostics of the weather.

IT is a well known general fact, that shepherds and others, whose occupations lie in the open air, do not unfrequently possess the skill of foretelling the weather for considerable periods of time in advance, and that they ground their observations upon the phenomena exhibited by animals, and other bodies exposed to the action of the elements. Some of the maxims on this subject, which possess the sanction of ancient acquiescence, are such as probably would not stand the test of modern scientific examination; but on the other hand, there are many of which the rationale appears not difficult to explain, and others which, from their empyrical value, are highly deserving to be studied and made out. I have for these reasons, as well as from a sense of the immediate utility of this knowledge, thought it by no means impertinent to beg that you would oblige the world with the enclosed, which is the best collection of facts I have met with. It is taken from a small pamphlet printed at Edinburgh, without date or the name of bookseller or author, but apparently many years ago, intitled, A Succinct Treatise of Popular Astronomy. If this communication should meet the honour of insertion in your excellent Miscellany, I hope it will be followed by some explanations from your scientific correspondents.

I am, Sir,

Your obliged reader,

R. B.

Signs

### *Signs of Rain from Birds.*

SEA and fresh water-fowls, such as cormorants, sea-gulls, muir-hens, &c. flying from sea, or the fresh waters, to land, shew bad weather at hand: land fowls flying to waters, and those shaking, washing, and noisy, especially in the evening, denote the same: geese, ducks, coots, &c. picking, shaking, washing, and noisy; rooks and crows in flocks, and suddenly disappearing; pyes and jays in flocks, and very noisy; the raven or hooded-crow crying in the morning, with an interruption in their notes, or crows being very clamorous at even; the heron, bittern and swallow flying low; birds forsaking their meat and flying to their nests; poultry going to roost, or pidgeons to their dove-house; tame fowls grubbing in the dust, and clapping their wings; small birds seeming to duck, and wash in the sand; the late and early crowing of the cock, and clapping his wings; the early singing of wood-larks; the early chirping of sparrows; the early note of the chaffinch near houses; the dull appearance of robin red-breast near houses; peacocks and owls unusually clamorous. Prognostics of the weather. Rain from birds.

### *Signs of Wind from Birds.*

Sea and fresh water-fowls gathering in flocks to the banks, and there sporting, especially in the morning; wild geese flying high, and in flocks, and directing their course eastward; coots restless and clamorous; the hoopoe loud in his note; the king's-fisher taking to land; rooks darting or shooting in the air, or sporting on the banks of fresh waters; and lastly, the appearance of the malefigie at sea is a certain forerunner of violent winds, and (early in the morning) denotes horrible tempests at hand. Wind from birds.

### *Signs of Fair Weather from Birds.*

Halcyons, sea-ducks, &c. leaving the land and flocking to the sea; kites, herons, bitterns and swallows flying high and loud in their notes; lapwings restless and clamorous; sparrows after sun-rise restless and noisy; ravens, hawks and kestrils (in the morning) loud in their notes; robin red-breast mounted high, and loud in his song; larks soaring high, and loud in their songs; owls hooting with an easy and clear note; bats appearing early in the evening. Fair weather from birds.

### *Signs of Rain from Beasts.*

Rain from beasts.

Asses braying more frequently than usual; hogs playing, scattering their food, or carrying straw in their mouths; oxen snuffing the air, looking to the south, while lying on their right sides, or licking their hooves; cattle grasping for air at noon; calves running violently and gamboling; deer, sheep, or goats, leaping, fighting or pushing; cats washing their face and ears; dogs eagerly scraping up earth; foxes barking, or wolves howling; moles throwing up earth more than usual; rats and mice more restless than usual; a grumbling noise in the belly of hounds.

### *Signs of Rain from Insects.*

Rain from insects.

Worms crawling out of the earth in great abundance; spiders falling from their webs; flies dull and restless; ants hastening to their nests; bees hastening home, and keeping close in their hives; frogs and toads drawing nigh to houses; frogs croaking from ditches; toads crying on eminences; gnats singing more than usual; but, if gnats play in the open air, or if hornets, wasps, and glow-worms appear plentifully in the evening, or if spiders webs are seen in the air, or on the grass, or trees, these do all denote fair and warm weather at hand.

### *Signs of Rain from the Sun.*

Rain, from sun.

Sun rising dim or waterish; rising red with blackish beams mixed along with his rays; rising in a musty or muddy colour; rising red and turning blackish; setting under a thick cloud; setting with a red sky in the east.

*N. B.* Sudden rains never last long; but when the air grows thick by degrees, and the sun, moon, and stars shine dimmer and dimmer, then it is like to rain six hours usually.

### *Signs of Wind from the Sun.*

Wind from the sun.

Sun rising pale and setting red, with an iris; rising large in surface; rising with a red sky in the north; setting of a bloody colour; setting pale, with one or more dark circles, or accompanied with red streaks; seeming concave or hollow; seeming divided, great storms; parhelia, or mock suns, never appear, but are followed by tempests.

Signs

### *Signs of Fair Weather from the Sun.*

Sun rising clear, having set clear the night before; rising while the clouds about him are driving to the west; rising with an iris around him, and that iris wearing away equally on all sides, then expect fair and settled weather; rising clear and not hot; setting in red clouds, according to the old observation: Fair weather from the sun.

*The evening red and morning grey,*
*Is the sure sign of a fair day.*

### *Signs of Rain from the Moon.*

Moon pale in colour, rain; horns blunt at first rising, rain; horns blunt, at or within two or three days after change, denotes rain for that quarter; an iris with a south wind, rain next day; wind south third night after change, rain next day; the wind south, and the moon not seen before the fourth night, rain most of that month; full moon in April, new and full moon in August, for most part, bring rain; mock-moons are the forerunners of great rains, land-floods, and inundation. Rain, from the moon.

### *Signs of Wind from the Moon.*

Moon seeming greatly enlarged; appearing of a red colour; horns sharp and blackish; if included with a clear and ruddy iris; if the iris be double, or seem to be broken in parts, tempests. Wind from the moon.

*N. B.* On the new moon, the wind for the most part changes.

When the moon, at four days old, has her horns sharp, she foretels a tempest at sea, unless she has a circle about her, and that too entire, because, by that she shews that it is not like to be bad weather, till it is full moon.

### *Signs of Fair Weather from the Moon.*

Moon seeming to exhibit bright spots; a clear iris with full moon; horns sharp fourth day, fair till full; horns blunt at first rising, or within two or three days after change, denotes rain for that quarter; but fair weather the other three quarters; moon clear three days after change or before full, always denotes fair weather. After every change and full, rains for the most part, succeeded by fair settled weather; moon clear and bright, always fair weather. Fair weather from the moon.

Signs

### *Signs of Weather from the Stars.*

Weather from stars.

Stars seeming large, dull, and pale of colour, rain; or when their twinkling is not perceptible, or if encompassed with an iris. In summer, when wind is at east, and stars seem greater than usual, then expect sudden rain; stars appearing great in number, yet clear and bright, seeming to shoot or dart, denote fair weather in summer, and in winter frost.

### *Signs of Rain from the Clouds.*

Rain from clouds.

In cloudy weather, when the wind falls, rain follows; clouds growing bigger, or seeming like rocks or towers setling on mountains tops; coming from the south, or often changing their course; many in number at north west in the even; being black in colour from the east, rain at night; but out of the west, rain next day; being many like fleeces of wool, from the east, rain for two or three days; lying like ridges about mid-day in the south-west, shews great storms both of wind and rain to be nigh.

### *Signs of Wind from the Clouds.*

Wind from clouds.

Clouds flying to and fro; appearing suddenly from the south or west; appearing red, or accompanied with redness in the air, especially in the morning; being of a leadish colour in the north-west; single clouds denote wind from whence they come; but if at sun-set, clouds appear with golden edges, or diminish in bulk, or small clouds sink low, or draw against the wind, or appear small, white, and scattered in the north-west (such as are vulgarly called mackerel) when the sun is high, these are signs of fair weather.

*N. B* It is often observed, that though the mackerel sky denotes fair weather for that day, yet for the most part, rain follows in a day or two after.

### *Signs of Rain from the Rainbow.*

Rain from rainbow.

After a long drought, the rainbow denotes sudden and heavy rains; if green be the predominant colour, it denotes rain, but if red, wind with rain; if the clouds grow darker, rain; if the bow seems broken, violent storms; if appearing at noon, much rain; if in the west, great rain, with thunder.

*N. B.*

*N. B.* It is obferved, that if the laft week in February, and the firft fortnight of March, be moftly rainy, and attended with frequent appearances of the bow, a wet fpring and fummer may be expected.

*Signs of Fair Weather from the Rainbow.*

The rainbow appearing after long rains, denotes fair weather at hand; if the colours grow lighter, fair; if the bow fuddenly difappears, fair; if the bow appear in the morning, it is the fign of fmall rains, followed by fair weather; if appearing at night, fair weather; if appearing in the eaft, in the evening, fair; if the bow appear double, it denotes fair weather at prefent, but rain in a few days; if in autumn, it continues fair for two days after the appearance of the aurora borealis, expect fair weather for at leaft eight days more. Fair from rainbow.

*Signs of Rain from Mifts.*

If mifts be attracted to the tops of hills then expect rain in a day or two; if in dry weather, they be obferved to afcend more than ufual then expect fudden rain; mifts in the new moon always forefhew rain in the old; mifts alfo in the old moon denote rains to happen in the new; a mifty white fcare, in a clear fky, in the fouth-eaft is always a forerunner of rain. Rain from mifts.

*Signs of Fair Weather from Mifts.*

If mifts diffipate quickly, or defcend after rain, it is a fure fign of fair weather; a general mift before fun rifing near the full moon, denotes fair weather for about a fortnight running. If after fun fet or before fun rife, a white mift arife from the waters and meads, it denotes warm and fair weather next day. A mifty dew on the infide of glafs windows fhews fair weather for that day. Fair from mifts.

*Signs of Rain from inanimate Bodies.*

Wood fwelling, or ftones feeming to fweat; lute or viol ftrings breaking; printed canvas or pafted maps relaxing; falt becoming moift; rivers finking, or floods fuddenly abating; remarkable fparkling of lamps or candles; remarkable halo about the candle; great drynefs of the earth; pools feeming troubled or muddy; yellow fcum on the furface of ftagnant waters; dandelion or pimpernel fhutting up; trefoil fwelling in ftalk, while the leaves bow down. Rain from inanimate bodies.

*N. B.* A dry fpring is always attended with a rainy winter.

Signs

### *Signs of Wind from inanimate Bodies.*

Wind from inanimate bodies.

Winds shifting to the opposite point; sea calm, with a murmuring noise; a murmuring noise from the woods and rocks; when the air is calm; leaves and feathers seeming much agitated; tides high when the thermometer is high; trembling or flexuous burning of flames; coal burning white with a murmuring noise; thunder in the morning with a clear sky; thunder from the north.

*N. B.* Whensoever the wind begins to shift, it will not rest till it comes to the opposite point; and, if the wind be in the north, it will be cold; if in the north-east colder; if in the south; it brings rain; but if in the south-west more rain.

### *Signs of Rain ceasing.*

Rain ceasing.

The sudden closing of gaps in the earth; the remarkable rising of springs or rivers; if the rain begins an hour or two before sun rise it is like to be fair ere noon; but if an hour or two after sun-rise, it for the most part, happens to continue all day and then to cease; when it begins to rain from the south with a high wind for two or three hours, and that the wind falls, and it still continues raining, it is then like to continue for twelve hours or more, and then to cease.

*N. B.* These long rains seldom happen to hold above twenty-four hours, or happen above once a year.

### *Signs of Wind ceasing.*

Wind ceasing.

A hasty shower after raging winds is a sure sign of the storm being near an end. If the water ruckles and frequent bubbles arise, or if the halcyon or kings-fisher attempts the sea while the storm lasts, or moles come out of their holes, or sparrows chirp merrily, these are all certain signs of the storm ceasing Both sea and fresh water fishes by their frequent rising and fluttering on the surface of the water, foretell the storm nigh over, but especially dolphins spouting up water in a storm foretell a calm.

*N. B.* Let the wind be in what quarter it will upon the new moon, it presently changes.

### *Signs of Hail.*

Hail.

Clouds white, inclining to yellow, and moving heavily though the wind be high is a sure sign of hail; if the eastern sky before sun-rise be pale, and refracted rays appear in thick clouds,

clouds, then expect great storms of hail; white clouds in summer are a sign of hail, but in winter they denote snow, especially when we perceive the air to be a little warm; in spring or winter when clouds appear of a blueish white, and expand much, expect small hail or drizzling, which properly is no other than frozen mists.

### *Signs of Thunder.*

Meteors shooting in the summer's evening, or chops and clefts in the earth, when the weather is sultry, always foretell thunder is nigh; in summer or harvest, when the wind has been south two or three days, and the thermometer high, and clouds rise with great white tops like towers, as if one were upon the top of another and joined with black on the nether side, expect rain and thunder suddenly; if two such clouds arise, one on either hand, it is then time to look for shelter, as the thunder is very nigh. Thunder.

*N. B.* It is observed that it thunders most with a south wind and least with an east.

### *Signs of Cold and Frosty Weather.*

Sea-pyes, starlings, fieldfares, with other migratory birds appearing early, denote a cold season to ensue; the early appearance of small birds in flocks and of robin red-breasts near houses; sun in harvest after setting in a mist or broader than usual; moon bright, with sharp horns, after change; wind shifting to the east or north after change; sky full of twinking stars; small clouds hovering low in the north; snow falling small, while clouds appear on heaps like rocks. Cold and frosty.

*N. B.* Frosts in autumn are always succeeded with rain.

### *Signs of Thaw.*

Snow falling in large flakes while the wind is at south; cracks appearing in the ice; sun looking waterish; the moon's horns blunted; stars looking dull; wind turning to the south; wind extremely shifting; it is also observed, that, if October and November be frost and snow, January and February are like to be open and mild. Thaw.

### *Signs of Drought.*

Fair weather for a week together, while the wind is all that time in the south, is, for the most part, followed by a great drought; Drought.

drought; if February be for moſt rainy, ſpring and ſummer quarters are like to be ſo too; but if it happen to be altogether fair, then expect a drought to follow; if lightning follow after twenty-four hours of dry and fair weather, drought will follow, but if within the twenty-four hours, expect great rains.

*Signs of Hard Winters.*

Hard winters.

A moiſt and cold ſummer, and mild autumn, are ſure ſigns of a hard and ſevere winter; ſtore of hips and haws denote the ſame; the hazel-tree flowering is ever obſerved to foretell the ſame; acorns found without any inſect is a ſure prognoſtic of a hard winter.

*Signs of Peſtilential Seaſons.*

Peſtilential ſeaſons.

A dry and cold winter with a ſoutherly wind; a very rainy ſpring, ſickneſs in ſummer; if ſummer be dry with the wind northerly but the autumn rainy and the wind ſoutherly, great ſickneſs is likely to follow; great heats in ſpring time without winds; roots having a luſcious taſte, while the wind has been long ſoutherly without rain; and laſtly, great quantities of ſtinking atoms, inſects or animals, as flies, frogs, ſnakes, locuſts, &c.

*Experiments of the Leech Worm.*

Leech worm.

Incloſe the leech worm in an eight ounce vial glaſs, three fourths filled with water covered with a bit of linen, let the water be changed once a week in ſummer, and once a fortnight in winter.

If the leech lies motionleſs at the bottom in a ſpiral form, fair weather; if crept to the top, rain; if reſtleſs, wind; if very reſtleſs, and without the water, thunder; if in winter at bottom, froſt; but, if in the winter it pitches its dwelling on the mouth of the vial, ſnow.

*Signs of Weather from the Barometer.*

Barometer.

In calm weather, when the air is inclined to rain, the mercury is low; but when tending to fair, it will riſe; in very hot weather when falling, it foreſhews thunder; if riſing in winter, froſt; but, if falling in froſt, thaw; if riſing in a continued froſt, ſnow; if foul weather quickly on its falling, ſoon over; if fair weather quickly on its riſing, ſoon over; alſo if riſing high in foul weather, and ſo continuing for two or three days, before

before the foul weather is over, then expect a continuance of fair weather; but, if in fair weather the mercury fall low, and so continue for two or three days, then expect much rain, and probably high winds.

*N. B.* In an east wind, the mercury always rises and falls lowest before great winds.

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## XVI.

*A New, Cheap and Simple Apparatus for repeating the Hours, and Quarters in Clocks and Watches. By Mr.* J.M. Elliot. *Communicated by the Inventor.*

THE great expence of repeating watches in the first purchase, as well as in the subsequent repairs, have induced Mr. Elliot to turn his thoughts towards reducing their construction to a greater degree of simplicity, and consequently to diminish the charges in both these respects. New and simple repeating motion.

In the mechanism represented in plates VII. and VIII. the parts and their arrangement are such that without the usual apparatus of pinions, pullies, chains, or racks, the watches repeat both the hour and the quarters, viz. the hour first and the quarter afterwards, or the hour or the quarters separately; or either of these first as may be wished, without interruption, and by one hammer. As several of these pieces have been made and disposed of in this country as well as exported, the practicability as well as the cost are both ascertained, which last is about one third of the former charges.

Plates VII. and VIII. represent the parts of this new machinery, *Fig.* 1. shews the principal pieces as attached to the upper pillar plate by means of a pottance A B C D, called the repeating pottance, kept to its place by the screw *x*.—*a b* shews the pendant attached to a steel axis upon which the repeating apparatus is fixed and carried. In the actual time piece these parts are as close to each other as convenience will allow; but they are here represented for the sake of distinctness, as if at a distance from each other. Thus they are made to act and perform the striking by turning the axis of the pendant to the right or left.

*c* is a part of the bell seen edgewise, *d* and *l* are two arms or pallets, which being fitted upon squares on the axis are

 moved

New and simple repeating motion.

moved round whenever the axis is moved by means of the pendant. The first *d* serves to drive the work for striking the hour, and the latter *l* the work for the quarters. The first of these apparatuses consists of *e* the hour-locking snail (see also *Fig.* 2.) which is fitted on the axis by a round socket or pipe, upon which socket also is fixed (steadily) the hour repeating wheel *g* (*Fig.* 1 and 2,) and (on a round part) the ratchet wheel *h*, which last is drawn by a spiral spring *k k* in the direction opposite to that which is produced by the pallet *d* (*Fig.* 1 and 2). *s* is the hammer urged by its spring *v* outwards, and the designer has, I am sorry to perceive, omitted to make a small tail from the center pin near *k* towards *g* the hour repeating wheel. Now the action of striking is simply this; when by turning the pendant *d* acts upon the small pin in *e*, the wheel *g* with its ratchet wheel *h* are carried round, and the teeth of *g* act on the tail of the hammer, and give more or fewer strokes, according to the number that pass.

The number is regulated thus, *a* (*Fig.* 4.) is the star-wheel as usual, and *b* the hour snail screwed on the same; *c* is the click with its back spring, and *n z* is the hour-locking lever, which terminates in an hook at *z* that may be seen in *Fig.* 2. bearing against the hour-locking snail *e*. This hook will evidently be nearer to or farther from the axis of the locking-snail *e*, according to the portion of the other end of the lever which bears against the hour snail *b*, and will consequently stop the pendant in its motion sooner or later by its opposition to one of the stops of the snail *e*. As the star wheel *a* is carried regularly round by the canon pinion as usual, its situation will determine the number of strokes.

The instant the pendant is let go, the spring *k* acts upon the ratchet wheel, and restores the original situation ready for striking again.

After this account of the hours, the striking of the quarters will require little explanation, *l* (*Fig.* 1 and 5.) is the pallet or arm acting (by a reverse motion of the pendant,) on the locking snail *m*, which carries the contrate wheel *p*, for striking the quarters by its action, upon another tail *r* of the hammer; and the original situation is restored by a ratchet wheel O and its spring, (see *Fig.* 6.)

In *Fig.* 4. is seen the quarter snail *g* with its lever *h k*, whose hook for regulating the number, may be seen at D in *Fig.* 5. and the snail is prevented from going back by a fixed hook.

## SCIENTIFIC NEWS.

### *Prize for the Artificial Compofition of Palladium.*

IN confequence of the intimation received by the anonymous paper of which a copy is given in the laft number of this Journal, at page 75, I waited upon Mrs. Forfter, who perfonally affured me that fhe holds the fum of twenty pounds, (which was paid to her by the fame unknown perfon who placed the palladium in her hands,) under the truft or engagement to pay the fame to the bearer of the certificate mentioned on the faid paper, at any time before Midfummer next; and that, if not claimed during that time, fhe fhall afterwards confider herfelf at liberty to repay the money when demanded by the fame unknown perfon.

I have therefore requefted Charles Hatchett, Esq. F. R. S. and Edward Howard, Esq. F. R. S. to join myfelf as judges of the product which may be made in our prefence, agreeably to the before-mentioned paper; to which propofal they have confented. And I have no doubt but that the commiffion will enable me to prefent to my readers an account of whatever may be the refult of this public invitation.

### *Prize adjudged at the laft Public Meeting of the National Inftitute*.*

THE decree of the government, May the 2nd. 1803, that anthorized the Inftitute to accept the capital of 10000 franks, [417 £.] offered by Lalande, expreffes, " that, agreeably to the intention of the donor, the intereft fhould be laid out annually in a gold medal, which, or its value in money, fhould be given to the perfon, in France or elfewhere, the members of the Inftitute only excepted, who fhould have made the obfervation, or written the memoir, that might appear moft conducive to the progrefs of aftronomy." Annual prize for the moft ufeful aftronomical paper or obfervation.

Accordingly, on the report of the committee nominated for the purpofe, the phyfical and mathematical clafs decreed the prize to Dr. W. Olbers, for having difcovered, in the courfe of the year 10, the planet to which aftronomers have given the name of *pallas olberfiana*. Decreed to Dr. Olbers for the difcovery of a planet.

* *La Decade Philofophique*. No. 29. July 1803. p. 142.

the

The Dr's. merit as an astronomer.

Before this discovery Dr. Olbers enjoyed considerable reputation among astronomers, for a dissertation on the most easy and commodious method of calculating the orbit of a comet; and for the laborious calculations and observations he had made, to re-discover Ceres, or Piazzi's planet, which in fact he did perceive, and was the first to announce to us.

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*Teylerian Society at Haarlem.**

Teylerian society's prize question.

THIS society has proposed the following question:

"Has the application of pretended supernatural principles to physics contributed to the progress of the science? or does not the history of this science prove on the contrary, that all the progress made in natural philosophy has been owing to observations, experiments, the conclusions drawn from them, and mathematical calculations and demonstrations?"

The prize is a gold medal of the value of 400 florins (£40), and the answers to the question, written in Dutch, Latin, French, English, or German, must be sent to the society before the 1st of April, 1804.

*** *The explanation of the subjects of the queries of R. B. in our last, (page 71.) are deferred for want of room.*

* *La Decade Philosophique*, No. 33. August, 1802. p. 382.

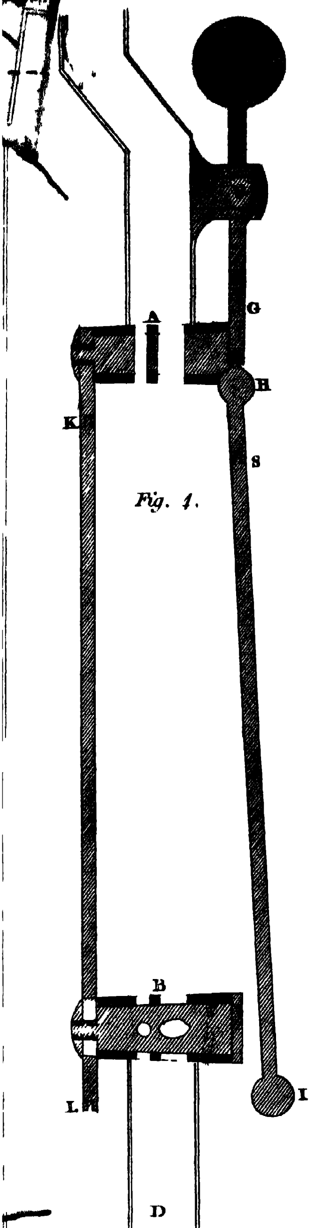



Fig. 4.

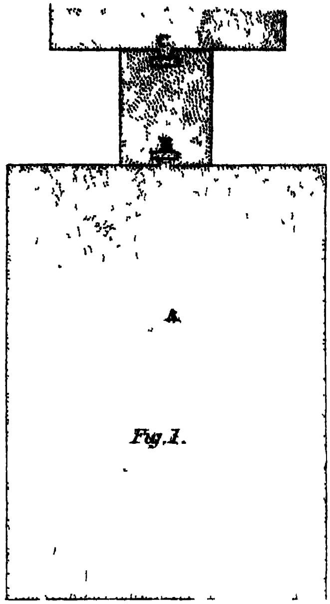



Fig. 1.

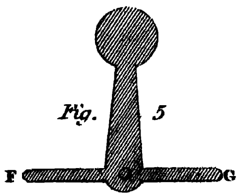



Fig. 5

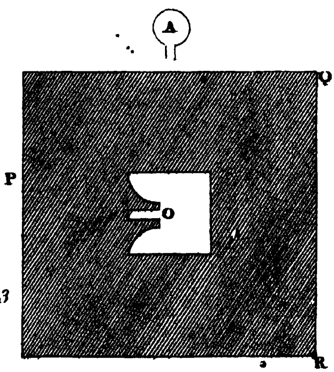



Fig 3

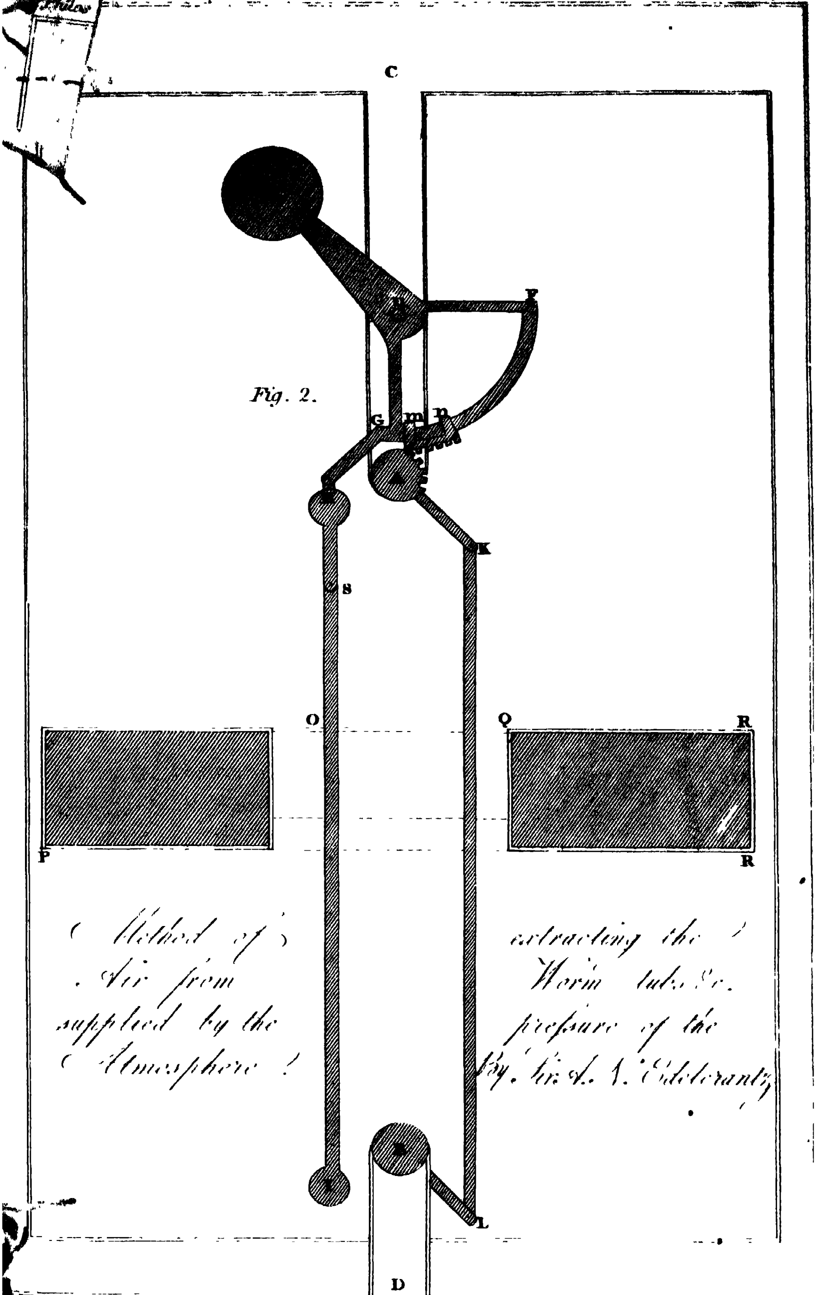



Fig. 2.

Method of extracting the Air from Worm tub &c. supplied by the pressure of the Atmosphere. By Mr. A. N. Edelcrantz

Mathew & Russell sc.

New & Simple Repeater,
by Mr. J. W. Elliot.

Fig. 1.

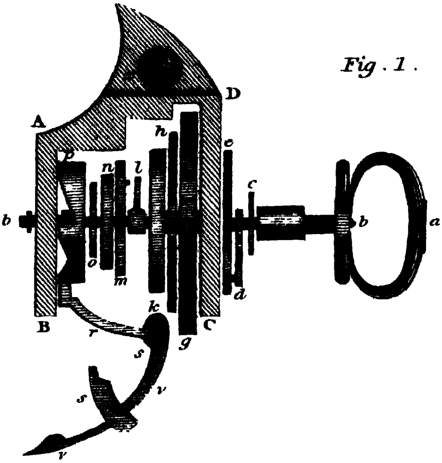



Fig. 2.

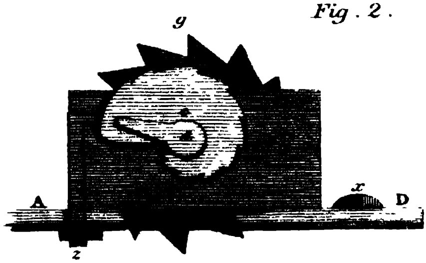
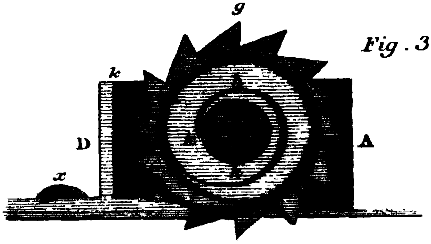



Fig. 3.




Drawn by Blunt

Engraved by Hatlow

Fig 4

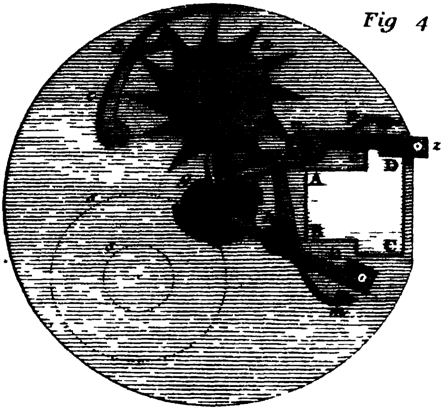



Fig. 5.

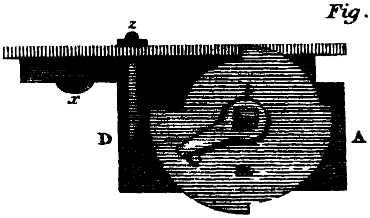
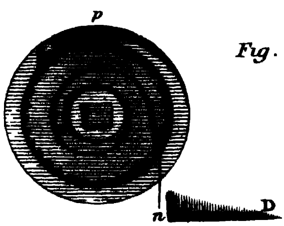



Fig. 6.




Drawn by Blunt

Engraved by Mutlow

A
# JOURNAL
OF
# NATURAL PHILOSOPHY, CHEMISTRY,
AND
# THE ARTS.

*MARCH*, 1804.

## ARTICLE I.

*Description of a new Steam Digester for Philosophical Researches. By Sir* A. N. EDELCRANTZ, *Counsellor of the Chancery, and private Secretary to the King of Sweden; Member of the Swedish Academy, &c. &c. Communicated by the Author.*

THE interesting and curious phenomena which this machine presented, from its first discovery by Papin, have always rendered it a desirable part of the chemical apparatus; but the difficulties and dangers attending its use, have, at the same time, prevented its applications from being so general, as the importance of the object seemed to promise. Many, and some of them successful, attempts have been made to adapt this instrument to economical and culinary uses; but these being commonly limited within very few degrees of heat above the boiling point, such digesters, though they can be used with safety, are at least inaccurate in the permanence, and uncertain in the determination of the elasticity and heat of the vapours acting in each experiment, which consequently cannot be either precise or comparable with others: and though several ingenious philosophers, such as Watt, Betancourt, Smith of Geissen, Van Marum, &c. have in later times very much improved their scientific application, enough of imperfection still seems to remain to prevent their general use, and to afford sufficient scope for further improvements.

Excellence and utility of Papin's digester.

Improvements.

As long ago as the year 1793 I applied my thoughts to render this inftrument lefs imperfect, and though I cannot flatter myfelf with having fucceeded in any confiderable degree, a fhort account of my attempts may not perhaps be thought quite ufelefs.

The ufual method of clofing the digefter is faulty in many refpects.

My firft object was to obtain a perfect clofure for the digefter, which is neceffary not only for increafing the heat, but alfo for preferving the contents. In former contrivances this has generally been effected by a metallic cover preffed on the top of the veffel by means of fcrews, wedges or other mechanical contrivances, with a circular piece of leather, paper, or other foft material between both. I alfo tried thefe methods in a great variety of ways, but always found fuch covers either leaky, on applying moderate preffures, or when the preffure was ftronger, too cumberfome for a courfe of varied and repeated experiments. Befides which, at the degree of 260 or 270° Fahr. the leather, as obferved before by Mr. *Betancourt*, is commonly decompofed or burnt, and linen or paper ceafe to clofe at a ftill lower temperature; as foon as the moifture, they are imbibed with becomes converted into vapour. I therefore ufed another cover of a particular kind, confifting of a thick circular plate, with its edge turned conical, and ground to fit exactly in the under fide of a metallic ring of the fame conical form, folded to the mouth of the digefter, in which the plate was previoufly included. When the veffel was ready for experiment and the plate lifted up fo as to apply to the ring, it clofed as exactly as could be defired, and the increafed force of the fteam, which tends to render other covers defective, had in this a contrary effect. Two difficulties were thus overcome, that of a cumberfome preffing apparatus, and the infufficiency of packings; but as the circular plate in this conftruction was made to remain conftantly in the digefter, I found that though it was laid near the fide of it, in fome experiments this mode was attended with an inconvenience which I was defirous of removing. I fucceeded by making another circular ring, turned with both fides conical, to be put between the circular plate and the opening of the digefter, fitting exactly to each of them. This ring *a b*, *Fig*. 5. *Plate* IX. paffes over the cover *c d*, when both are lowered, the ring remaining in the digeftor; but the circular plate is taken out through the enlarged opening *e f*. Though

New cover which fits by a conical interior furface, without packing.

Inconvenience of this contrivance.

I had

I had by this contrivance,* in a great meafure diminifhed the inconveniences, yet the intermediate ring remaining in the veffel, and the increafed difficulty of executing a more complicated inftrument, urged me to attempt fome other more expeditious mode of clofing.

Simple and moft accurate clofure.

I fhould almoft be afhamed to confefs that the moft fimple and, as I prefume, the beft contrivance of all, did not occur to me before I had tried the more complicated ones; if many refpectable examples had not proved that the eafieft and fhorteft method is often the moft difficult to find. The reafon is obvious: Simplicity, like truth, is only one; the bye paths of error are innumerable.

Defcription of the fame. It is a plate which fits againft a furface within.

This method confifts in making the cover fimply an oblong, fquare, or oval plane brafs plate, accurately ground againft the inferior furface of a fimilar frame of the fame metal, fixed and foldered to the opening of the digefter. This frame has an oblong aperture, through which the cover may be put in or taken out at pleafure. When put in and preffed gently againft the frame, it will by the means of a little oil,† have adhefion enough to bear its own weight, till the fteam begins to act, which will clofe it perfectly. But for greater fecurity a fmall piece of wood, fixed with a fcrew, will keep the plate joined to the frame, and ferve at the fame time as a handle. *Fig.* 1. *Plate* IX. reprefents the longitudinal fection of the whole inftrument, with all its parts; *a b* is the covering plate, ground and fitted againft a fquare metallic frame *c d*, foldered ‡ to the veffel at *e f*, which ought to be made of hammered copper at leaft 1½ line thick. *Fig.* 2 is the tranfverfe fection of the fame apparatus, and *Fig.* 3. a plan of the brafs plate, where the dotted lines mark the opening of the frame *c d*, *Fig.* 1. and the

* The digefter with a conical plate, and the fafety-pifton defcribed below, firft executed in Stockholm, 1793, was exhibited to the members of the Royal Society in Berlin, in Feb. 1802; and a defcription of it, printed in the Obfervations Phyfiques of Mr. Delametherie, Feb. 1803.

† The different force of adhefion of the fame metal, in employing different oils, is remarkable, and may afford a field for future inveftigation.

‡ Befides foldering, the parts fhould be joined together with rivets or fcrews.

part covered by the fame. *Fig.* 4. *h i* is the wooden handle turning round the fcrew *g*, *Fig.* 1, 2. to connect the metallic planes. It is a kind of button or key.

Peculiar fafety valve by which the preffure may be regulated.

My next object was to render the inftrument fafe, and to place the operator's mind independent of any apprehenfion of fatal accidents. For this purpofe, (befides the ftrength of the veffel and its different parts, which ought to be as folid as conveniently can be made,) a metallic cylinder *m n* is fixed to the plate *a b*, to the inferior open extremity of which a metallic fphere *m b*, perforated with fmall holes, adheres by mere friction, allowing the vapour, but not the folid or other matters, to pafs into the cylinder, which contains a folid pifton *k o*, made of the fame metal * and exactly ground into it, fo as to fink down by its own weight if lifted, without however permitting the vapour to efcape. This vapour when heated to a certain degree, will raife the pifton above one or more of the capillary holes *s s*, on the fide of the cylinder and then efcape. The pifton will remain ftationary or ofcillating at a height, where the quantity of fteam efcaping is equal to the quantity produced in the fame time by that fire, confequently at a low point if the fire is weak, or, if the fire is intenfe, at the top, where a larger hole prefents a compleat iffue to the fteam and prevents the poffibility of accidents.

Conditions for accurate experiment with the digefter. 1. To know the temperature. 2. To command the heat, and 3. To render it fteady though the fire be not fo.

To adapt the inftrument thus conftructed to exact and comparative philofophical experiments, three things feem to be required: 1. to afcertain the degree of heat of the interior fluid contents † by a thermometer; 2. to increafe that heat at pleafure; and 3. to fix it invariably, if it be defired, independent of the poffible increafe of the fire, during the courfe of the experiment.

The temperature meafured by a thermometer.

The firft is obtained by means of a fmall iron cup *p q*, folded into the plate, and containing fome mercury. A fine thermometer paffed through a cork, being fixed in the cup, the

* Great care fhould be taken to make both of the fame metal, for in one inftance when I had the pifton made of the fame brafs with the cylinder, but of another day's cafting, this infinitely fmall difference was ftill fufficient to produce a different force of expanfion, in confequence whereof the pifton, which moved freely at 212°, ceafed to move, and was quite faftened at 260°.

† It is perhaps fuperfluous to obferve that a digeftor is only intended for experiments with fluids or bodies furrounded with them, and confequently never contains materials in a dry ftate.

degree

degree of heat is eafily obferved on the ftem. This degree will not be exactly the fame with that of the interior fpace; it fhews always fomewhat lefs, which fact was long time ago obferved by Mr. *Braun* of the academy of Peterfburg; but the difference is fmall, and being once afcertained, by experiment made on purpofe, a due correction, by adding that difference, is eafily made.

The heat given by a lamp.

The heat is communicated in my apparatus by a common Argand's lamp, which was always found fufficient and of which the effect may be regulated by the pinion of that inftrument. If time be an object, or if the digefter is much more than double the fize of *Fig*. 1. the fcale of which is one third of the real fize, two fuch lamps may be applied. But a lamp with fpirit of wine and three wicks will fupply fire enough for a temperature of 270 or 280 if the digefter be fmall and exactly clofed. As the atmofphere continually takes away a part of the heat communicated to the veffel, it is only the difference of the communicated and diffipated heat which acts on the fteam, and determines the limits of its force, when the fire is weak, and the digefter of a large fize. On the other fide, the heat of the interior fluid and the elafticity of fteam correfponding to it, cannot, after having lifted the pifton, be encreafed by a continued fire or by a greater number of lamps, but remains from that moment conftant, which feems to be a particular property of this conftruction. A farther *increafe* of heat, as the 2d. defideratum, can only be obtained by oppofing a greater refiftance to the action of the vapour beneath the furface *k*, which is performed by loading the pifton with one or more of the weights *r r*, *Fig*. 1. Thus we may either add new weights progreffively, till the thermometer indicates the degree of heat required, or knowing by the former experiments of philofophers the correfponding ratio of increafed preffure and heat of elaftic fluids, we can load the pifton at once with a weight proportional to the intended heat and the furface of its bafe *k*. This will then rife as foon as that degree is produced, and allowing the fteam to efcape from one or more of the holes * prevent any

The heat is conftant if the preffing weight be not changed.

What is the fecond condition.

* By what is faid above it is evident that the height of the pifton, or the number of the holes open to the fteam, is of no confequence, it being always compreffed by the fame weight, and acting with the fame elafticity. The *quantity* produced is only different.

any further increafe of the heat, which will remain *invariable* as the third defideratum required.*

Very great difadvantage of imperfect clofure of the digeftor.

Here it feems proper once again to infift upon the great importance of a perfect clofure in this inftrument. If the heated fteam, by inaccuracy in the conftruction, is permitted to efcape, not only a great part of the contents is wafted without any ufe, before the experiment can be faid to begin, which fometimes obliges us to open the digefter and fill it a fecond time, but even fome of the materials may be altered and decompofed before the intended effect can take place. Still more, very often in fuch cafes, a ftrong kitchen fire, with the greateft exertions of the operator, (not to mention the trouble and inconvenience of fuch an operation) is infufficient to afford the degree of heat, which a fingle lamp, placed with the apparatus on a fmall writing table, would produce with eafe, and without wafte of the materials.

In the prefent ftate of natural philofophy, the progrefs of the fcience feems to depend in a great meafure on the perfection of our inftruments. In this point of view the preceding attempts to render a little ufed but very excellent machine more fafe, exact, and convenient †, may, if it fhould not anfwer that purpofe, at leaft give rife to other more fuccefsful improvements.

* A fhort defcription of this contrivance was prefented to the *Philofophical Magazine* in June, 1803, and publifhed in December the fame year; but by the ufe of the word *pifton* inftead of *digefter* in a note, an erroneous ftatement is given in the printed account, which, however trifling it may appear, it is proper to correct, as this part of the inftrument has not been changed at all, fince it was firft made and known in Sweden ten years ago. In the fame paper I propofed the ufe of it for regulating the fire in boilers of fteam engines, &c.—and its effect as a *regulator,* both of the *power* of the *fteam,* and the *intenfity* of the *fire,* depends entirely upon the *weight* being conftant and *invariable* in the whole motion of the pifton.

† Digefters, as defcribed in this paper, are made by Mr. FIDLER, mathematical inftrument maker, 23, Oxford-market, London.

II. *Inveftigation*

## II.

*Investigation of certain Theorems relating to the Figure of the Earth. By* JOHN PLAYFAIR, *F. R. S. Edin. and Professor of Mathematics in the University of Edinburgh.*

*(Concluded from Page 116.)*

21. A REMARK, that is in no danger of being reckoned hypothetical, is, that the conclusion derived from the comparison of degrees of the meridian, with degrees of the circle perpendicular to it, becomes of necessity more liable to error as we advance into higher latitudes. The reason is, that whatever error is committed in determining the magnitude of $D' - D$, must be multiplied into the square of the secant of the latitude, in order to give its full effect in changing the value of the fraction $\frac{c}{a}$. For it has been shewn that $\frac{c}{a} = \frac{1}{2}\left(\frac{D' - D}{D'}\right)$ sec $^3\phi$; now, if we suppose the error committed in ascertaining $D' - D$ to be in all cases the same, the error of the fraction $\frac{D' - D}{D'}$ will also be in all cases nearly the same, the denominator $D'$ being but little affected either by the supposed error, or by the change of latitude. But this error, which may thus be considered as a constant quantity, when multiplied into $\frac{1}{2}$ sec $^2\phi$, gives the variation or error in $\frac{c}{a}$, which error therefore increases, *cæteris paribus*, as the square of the secant of the latitude, so that, on approaching the pole, it increases without limit, and is ultimately infinite. Comparisons of this kind may therefore be expected to give results the more accurate the nearer they are to the equator, under which circle they will be the most accurate of all. Here, again, however, another circumstance must be taken into consideration, viz. that the method of ascertaining the differences of longitude by the convergency of the meridians, so convenient in surveys of this kind, is applicable only in high latitudes. In a trigonometrical survey, therefore, of a country lying much farther south than Britain, a different method of ascertaining the longitudes of places must necessarily be adopted.

This method by the meridian and its perpendicular is more erroneous the higher the latitude.

22. The

To determine the figure of the earth from measures of the perpendicular in different latitudes.

22. The theorems, which were next propofed to be confidered, are thofe that determine the figure of the earth from the meafures of degrees of the curve perpendicular to the meridian, in different latitudes. For this purpofe let D′ be a degree of one of thefe curves, in the latitude $\phi'$, and D″ a degree of one of them, in another latitude $\phi''$. Then $c$ being the compreffion, as before, we have by § 18. $mD' = a + c \text{ fin }^2\phi'$,

and alfo $mD'' = a + c \text{ fin }^2\phi''$.

Hence $m(D' - D'') = c(\text{fin }^2\phi' - \text{fin }^2\phi'')$, and

therefore $c = \frac{m(D' - D'')}{\text{fin }^2\phi' - \text{fin }^2\phi''}$.

This formula may be rendered more convenient for calculation, by confidering that $\text{fin }^2\phi' = \frac{1 - \text{cof } 2\phi'}{2}$, fo that

$\text{fin }^2\phi' - \text{fin }^2\phi'' = \frac{1 - \text{cof } 2\phi' - 1 + \text{cof } 2\phi''}{2} =$

$\frac{\text{cof } 2\phi'' - \text{cof } 2\phi'}{2}$. But $\text{cof } 2\phi'' - \text{cof } 2\phi' = 2 \text{ fin } (\phi' + \phi'') \times$ $\text{fin } (\phi' - \phi''$, wherefore $\text{fin }^2\phi' - \text{fin }^2\phi'' = \text{fin } (\phi' + \phi'') \times$ $\text{fin } (\phi' - \phi'')$, and $c = \frac{m(D' - D'')}{\text{fin } (\phi' + \phi\ ) \times \text{fin } (\phi' - \phi')}$.

23. In the fame manner, becaufe $mD' = a + c \text{ fin }^2\phi'$, by fubftituting for $c$, we have

$$mD' = a + \frac{m(D' - D'') \text{ fin }^2\phi}{\text{fin } (\phi' + \phi'') \times \text{fin } (\phi' - \phi'')}, \text{ and}$$

$$a = mD' - \frac{m(D' - D'') \text{ fin }^2\phi}{\text{fin } (\phi' + \phi'') \times \text{fin } (\phi' - \phi'')}.$$

24. Laftly, fince $mD' = a + c \text{ fin }^2\phi'$,

and $mD'' = a + c \text{ fin }^2\phi''$,

dividing the firft of thefe equations by the fecond, and rejecting the higher powers of $c$, we have

$\frac{D'}{D''} = 1 + \frac{c}{a}(\text{fin }^2\phi' - \text{fin }^2\phi'')$, and therefore,

$\frac{c}{a} = \frac{\frac{D'}{D''} - 1}{\text{fin }^2\phi' - \text{fin }^2\phi''}$. Hence alfo

$\frac{c}{a} = \frac{\frac{D'}{D''} - 1}{\text{fin } (\phi' + \phi'') \times \text{fin } (\phi' - \phi'')}$; or more conveniently for calculation by logarithms, $\frac{c}{a} = \frac{D' - D''}{D'' \text{ fin } (\phi' + \phi'') \times \text{fin } (\phi' - \phi'')}$.

25. We

25. We may compare this value of $\frac{c}{a}$ with that obtained in § 18. from other data, in order to determine which of the two methods of finding $\frac{c}{a}$ is to be preferred, under given circumstances. Suppose, for instance, a degree of the curve perpendicular to the meridian in the latitude $\phi'$ to be $D'$, and a degree of the meridian itself in the same latitude to be $\Delta'$; it is required to find in what other latitude $\phi''$, a degree $D''$, perpendicular to the meridian, must be measured, in order that the comparison of $D'$ and $D''$, and of $D'$ and $\Delta$, may give values of $\frac{c}{a}$, in which the probable error is the same.

To determine the figure of the earth from measures of the perpendicular in different latitudes.

Here, agreeably to an observation already made, we may, in order to estimate the error produced in $\frac{c}{a}$, in consequence of an error in the determination of $D'$, and $D''$, and $\Delta$, suppose the error to affect $D' - D''$, or $D' - D''$ only, without paying any regard to the variation of $D'$ in the denominator. Therefore, since by § 18 we have $\frac{c}{a} = \frac{D' - \Delta}{2D' \cos^2\phi'}$, and again by § 24, $\frac{c}{a} = \frac{D' - D''}{D''(\sin^2\phi' - \sin^2\phi'')}$, if we suppose equal errors in determining $D' - \Delta$, and $D' - D''$, and also that these are the only errors, their effect will be the same, in both cases, if $2 \cos\phi'^2 = \sin(^2\phi' - \sin^2\phi'')$. Now, if we suppose $\phi''$ the quantity sought, and add $\cos\phi'^2$ to both sides of the preceding equation, then $3 \cos^2\phi' = \sin^2\phi' + \cos^2\phi' - \sin^2\phi'' = 1 - \sin^2\phi'' = \cos^2\phi''$. The latitude $\phi''$ therefore must be such, that $\cos\phi'' = \sqrt{3} \times \cos\phi'$. If, therefore, $\phi'$ be such that $\cos\phi' = \frac{1}{\sqrt{3}}$, the cosine of $\phi''$ will be $= 1$, and $\phi''$ therefore $= 0$. Now, $54° 44'$ is the arch of which the cosine $= \frac{1}{\sqrt{3}}$ nearly, therefore if a degree of the meridian, and of the perpendicular to it, be measured in latitude $54° 44'$, the comparison of these with one another will give a result as accurate as if the degree of the perpendicular, in that latitude, were compared with the degree at the equator, and more accurate of consequence, than if any other degree of the perpendicular to the meridian, were to be compared with $D'$.

26. Hence,

To determine the figure of the earth from measures of the perpendicular in different latitudes.

26. Hence, alfo, the comparifon of the degree of the meridian, and of the perpendicular to it, in the fouth of England, is better than if a degree of the perpendicular meafured in that latitude were compared with a degree at the equator. For if, in the equation cof $\phi'' = (\text{cof } \phi') \times \sqrt{3}$, we make $\phi' = 50^\circ\ 41'$, (or any thing lefs than $54^\circ\ 44'$,) $\phi''$ will come out impoffible.

27. It may be fhewn, too, nearly in the fame manner, that if a degree of the perpendicular to the meridian were meafured in Siberia, as far north as the latitude of 70°, fuppofing that to be poffible, and compared with a degree in latitude 45°, or even confiderably farther fouth, it would not give a refult fo exact as the degree of the meridian and perpendicular meafured in the fouth of England. This fhews, that the method of afcertaining the figure of the earth, propofed by the authors of the *Trigonometrical Survey, (Phil. Tranf.* ibid. p. 529) as a fubject of future inquiry, is lefs exact than that which is founded on their own obfervations.

Whether the comparifon of a degree of the merid. and perp. in fame lat. be more accurate than that of merid. degs. in different lats.

28. We may alfo afcertain, by the fame means, the relative accuracy of the method of finding the figure of the earth, from the comparifon of a degree of the meridian with a degree of the perpendicular in the fame latitude, and of the method of refolving the fame problem by the comparifon of two degrees of the meridian in different latitudes.

If, then, D be a degree of the meridian, and D′ of the perpendicular, in latitude $\phi$, and if $\Delta$ be a degree of the meridian in a different latitude $\phi'$, it is required to find whether the moft accurate value of $\frac{c}{a}$ will be found, by comparing D and D′, or D and $\Delta$.

Since we have, by what has been already ftated, § 4.

$$mD = a - 2c + 3c \text{ fin }^2\phi, \text{ and}$$

$$m\Delta = a - 2c + 3c \text{ fin }^2\phi', \text{ we have alfo}$$

$$\frac{D}{\Delta} = 1 + \frac{3c}{a}(\text{fin }^2\phi - \text{fin }^2\phi') \text{ and therefore.}$$

$$\frac{c}{a} = \frac{D - \Delta}{3\Delta(\text{fin }^2\phi - \text{fin }^2\phi')}.$$

Now, it has been already fhewn, that, by comparing D and D′ we have $\frac{c}{a} = \frac{D' - D}{2D' \text{ cof }^2\phi}$. Suppofing, therefore, equal errors to be committed in the determination of $D - \Delta$, and of

D′

$D' - \dot{D}$, and alfo paying no regard to the inequality of $\Delta$ and $D'$ in the denominators of thefe fractions, as it is not fo great as materially to affect the quantity that is fought for here, we fhall have the errors in $\frac{c}{a}$ nearly the fame in both formulas, when $\phi$ and $\phi'$ are fuch that $2 \cos^2\phi = 3 \sin^2\phi - 3 \sin^2\phi'$, or when $\frac{2}{3} \cos\phi^2 = \sin^2\phi - \sin^2\phi'$, that is, adding $\cos^2\phi$ to both fides,

$\frac{5}{3} \cos^2\phi = \sin^2\phi + \cos^2\phi - \sin^2\phi'$, and, therefore,

$\frac{5}{3} \cos^2\phi = 1 - \sin^2\phi' = \cos^2\phi'$, or $\cos\phi' = (\cos\phi)\sqrt{\frac{5}{3}}$.

29. If, therefore, $\cos\phi = \sqrt{\frac{3}{5}}$, $\cos\phi' = 1$, that is $\phi' = 0$, fo that $\Delta$, the fecond of the degrees of the meridian, muft in this cafe be under the equator. But $\sqrt{\frac{3}{5}}$ is the cofine of 39° 14′, in which latitude therefore if D and D′ be meafured, the refult, by comparing them with one another, is as exact as if D were compared with the degree under the equator. Hence, if D and D′ are meafured in a lower latitude than the above, the refult will be more exact, than if D were compared with the degree at the equator.

If we fuppofe D and D′, meafured in the fouth of England, fo that $\phi = 50° 41'$; then we will have $\phi' = 35° 7'$, fo that D muft be compared with a degree of the meridian as far fouth as 35° 7′, in order that the refult may be as good as when D and D′ are compared with one another.

From this it is evident, that the method of comparing degrees of the meridian, and perpendicular in the fame latitude, has even an advantage over the comparifon of degrees of the meridian in different latitudes, unlefs thefe laft are taken at a confiderable diftance from one another.

The former method is preferable unlefs the degs. of the mer. be far afunder.

In this way may many ufeful conclufions be derived concerning the degree of credit due to meafurements already made, as well as with refpect to the felection of the places where they are to be made hereafter. On thefe I fhall enter no further at prefent, and fhall only add, that, befides the advantages or difadvantages which the method of comparing together degrees of the meridian and perpendicular in the fame latitude has, and which are fubjects of calculation, it has another

The former method alfo fuppofes the fame obfervers and the fame inftruments.

other advantage, which in the cafe of the Britifh furvey is undoubtedly very great, viz. that all the *data* are furnifhed from one fyftem of trigonometrical operations; executed according to the fame plan, with the fame inftruments, and by the fame obfervers.

To determine the earth's figure by comparing an arc of the mer. with that of a parellel in fame lat.

30. One other application of geometrical meafurements to difcover the figure of the earth yet remains to be confidered. This is the comparifon of an arch of the meridian with an arch of a parallel of latitude which croffes it. The meafure of a parallel of latitude can be executed readily, and is not confined to a fmall arch, as in the cafe of a perpendicular to the meridian. The plumb-line, while it is carried along the circumference of a parallel to the equator, tends continually to the fame point in the earth's axis, fo that there is no difficulty in afcertaining the amplitude of the arch meafured, providing there be no unufual difturbance of the direction of gravity. As an arch of a parallel to the equator, however, is not the fhorteft line between two points on the furface of the fpheroid, the meafurement along that furface will not give the length of the arch truly. To obviate this difficulty, it is only neceffary to follow the method fo properly introduced into the *Trigonometrical Survey*, of reducing the meafures, both of lines and angles, to the chords and to the planes of the rectilineal triangles contained by them. In this way, the chord of an arch of a parallel of latitude may be determined, however great the arch; and it is worthy of being remarked, that, whatever be the deflections of the plumb-line at the intermediate ftations, when the reductions are all properly made, the length of the chord meafured will not be affected by them; the amplitude of the arch indeed may be affected by fuch deflections, if they happen at its extremities; but the effect of this error will be rendered the lefs, the greater the arch that is meafured. We may fuppofe, therefore, that the chord of a large arch of a parallel of latitude is meafured, and the amplitude of the arch itfelf at the fame time accurately afcertained. This laft may be done, either by meafuring the convergency of the meridians, if it be in a high latitude, or by any other method of afcertaining differences of longitude which admits of great accuracy. The chord being thus given in fathoms, and the arch fubtended by it being given in degrees and minutes, the radius of the parallel itfelf becomes known.

31. Now,

31. Now, if we would compare the radius of a parallel thus found, with a large arch of the meridian, we ſhall have by that means a determination of the figure of the earth, not leſs to be relied on than that given in the beginning of this paper. The inveſtigation is eaſy by help of the theorems in § 5 and 6. Let FO be the radius of a parallel to the equator, which paſſes through F, the latitude of which is $\phi$, and is ſuppoſed known; and let FO found by the method juſt deſcribed be $= r$, then, as in § 4. $r = \frac{a^2 \text{coſ} \phi}{\sqrt{a^2 \text{coſ} \phi^2 + b^2 \text{ſin} \phi^2}} = \frac{a \text{ coſ} \phi^2}{a\sqrt{1 - \frac{2c}{a} \text{ſin} \phi^2}}$, according to the method of reduction followed in the preceding articles of this paper. Then, becauſe $\sqrt{1 - \frac{2c}{a} \text{ſin} \phi^2} = 1 + \frac{c}{a} \text{ſin} \phi^2$ nearly, we have $r = a \text{ coſ} \phi \cdot (1 + \frac{c}{a} \text{ſin} \phi^2) = a \text{ coſ} \phi + c \text{ ſin} \phi^2 \text{ coſ} \phi$, or if we divide by coſ $\phi$, $\frac{r}{\text{coſ} \phi} = a + c \text{ ſin} \phi^2$. Let $\frac{r}{\text{coſ} \phi} = l$, then $l = a + c \text{ ſin} \phi^2$.

To determine the earth's figure by comparing an arc of the mer. with that of a parallel in ſame lat.

32. Again, if $\phi'$ and $\phi''$ are the latitudes of the extremities of an arch of the meridian, the length of which has been meaſured, and found $= l'$, then, according to § 5 we have

$$l' = a(\phi'' - \phi') - \frac{c}{2}\left((\phi'' - \phi') + \frac{3}{2}(\text{ſin } 2\phi'' - \text{ſin } 2\phi')\right).$$

If, therefore, $m$ be the coefficient of $a$, in the former equation, and $n$ the coefficient of $c$; and if $m'$ be the coefficient of $a$, in the latter equation, and $n'$ of $c$, we have, as in § 6.

$$a = \frac{n'l - nl'}{mn' - m'n}, \text{ and } c = \frac{m'l - ml'}{mn' - m'n}, \text{ or ſince } m = 1,$$

$$a = \frac{n'l - nl'}{n' - m'n}, \text{ and } c = \frac{m'l - l'}{n - m'n}; \text{ alſo } \frac{c}{a} = \frac{m'l - l'}{n'l - nl'}.$$

33. In this way of determining $a$ and $c$, the parallel of latitude may either interſect the arch of the meridian meaſured or not. If it interſect that arch, this method may have the ſame advantage that was taken notice of in another ſolution, viz. that the whole of the *data* may be furniſhed from the ſame ſyſtem of trigonometrical operations. Thus, in the ſurvey of Great Britain, an arch of five or ſix degrees of a parallel to the equator might be meaſured, and compared with the whole length

To determine the earth's figure by comparing an arc of the mer. with that of a parallel in fame lat.

length of the meridian, comprehended between the northern and fouthern extremities of the ifland, amounting nearly to nine degrees.

It is plain, from what has already been faid, that the refult deduced from this comparifon would poffefs every advantage, and would be entitled to more credit, than any determination of the figure of the earth that is yet known.

34. On the fuppofition that, in a furvey of a country, the meafurement is made along a feries of triangular planes, all given in pofition and magnitude, there is yet another method of determining the figure of the earth, more general than any of the former. On the fuppofition juft mentioned, it is evident, that the length of a ftraight line or chord, drawn from a given angle of any one of thefe triangles, to a given angle of any other of them, may be found by trigonometrical calculation. Let the latitudes be obferved at the extremities of this chord, and alfo the difference of longitude; then, from the nature of an ellipfoid, the length of this fame chord may be expreffed, in terms of the axes $a$ and $b$, together with the latitudes of the extremities of the chord, and the difference of longitude between them; and this expreffion being put equal to the length of the chord meafured, will give an equation, in which all the quantities are known, except $a$ and $b$. Further, if $a = b + c$, and if the faid expreffion be reduced into a feries, with the powers of $c$ afcending, that feries will converge very rapidly, becaufe $c$ is fmall in refpect of $a$; then, for a firft approximation, we may reject all the terms that involve the powers of $c$ higher than the firft, by which means we fhall have a fimple equation of the form $ma + nc = l$, where $m$ and $n$ are functions of the latitudes and difference of longitude, and $l$ is the length of the chord.

Now, if a fimilar equation be derived from the meafurement of any other chord, thefe two equations will give $a$ and $c$ in the fame manner as in § 6; and thus, from the meafurement of any two chords, the figure of the earth will be determined.

35. The length of the chords, thus meafured, fhould be great, fo that they may, if poffible, fubtend angles of feveral degrees, and their pofition will be moft favourable when one of them is in the plane of the meridian, and the other nearly at right angles to it. The numerical computation will be found lefs laborious than might be imagined; but the complete

plete ſolution of the problem, and the full detail of the inveſtigation, I am under the neceſſity of delaying to ſome future communication.

To determine the earth's figure, by a general method with triangles.

There ſeems to be but one difficulty of any conſequence that ſtands in the way of this method of determining the figure of the earth. It ariſes from this, that the aſcertaining the poſition of the ſuppoſed ſeries of triangular planes relatively to one another, involves in it the allowance to be made for the terreſtrial refraction, which it muſt be confeſſed is not accurately known, and is the more difficult to determine, that it is unavoidably combined with the irregularities in the direction of gravity. It is poſſible, indeed, to ſeparate theſe two ſources of error, but not without a ſyſtem of experiments inſtituted directly for that purpoſe.

36. The determination of the difference of longitude, which enters neceſſarily into this problem, except in the caſe when both chords are in the direction of the meridian, muſt alſo be performed with great accuracy. Among the different ways of doing this, that which proceeds by obſerving the convergency of the meridians, though the beſt accommodated to the nature of a trigonometrical ſurvey, is not the leaſt liable to objection. For, not to mention that it is only practicable in high latitudes, we muſt obſerve, that it always implies a correction on account of the ellipticity of the meridian, which is therefore neceſſarily hypothetical, and depends on the very thing that is to be found. This inconvenience, however, may be obviated by repeated approximations, and by an accurate ſolution of *ſpheroidal* triangles. On this latter ſubject it was my intention to offer to the Society ſome theorems, that contain more direct and fuller rules for this kind of trigonometry than any that I have yet met with. I am under the neceſſity, however, of reſerving theſe, as well as the ſolution of the problem above-mentioned, for the ſubjects of ſome future communication. In the mean time, I think it is material to obſerve, that the principle laid down by Mr. Dalby, viz. that in a ſpheroidal triangle, of which the angle at the pole and the two ſides are given, the ſum of the angles at the baſe is the ſame as in a ſpherical triangle, having the ſame ſides, and the ſame vertical angle, is not ſtrictly true, unleſs the eccentricity of the ſpheroid be infinitely ſmall, or the triangle be very nearly iſoſceles. The application of the principle may therefore lead into error, unleſs it be made with due

attention to these restrictions. The gentleman, just named, will forgive a remark, which I certainly should not have made, if I had been less interested for the success of the work, in which he has assisted with so much ability.

## III.

*Enquiries concerning the Nature of a Metallic Substance lately sold in London as a new Metal, under the Title of Palladium. By* RICHARD CHENEVIX, *Esq. F. R. S. and M. R. I. A.**

*(Concluded from page* 101.*)*

(Concluded from page 101.)

EXPERIMENTS TO PROVE AFFINITY AMONG THE METALS.

Instances of affinity in metals silver precip. by muriate of platina carried down 21 per. cent. of the metal.

*Exp.* 1. I dissolved one hundred grains of silver in nitric acid, and precipitated by neutral muriate of platina. The precipitate, well washed and dried, was of a bright straw-colour, and weighed 147 grs. Reduced in a charcoal crucible, it yielded a button weighing 121 grs. and of the specific gravity of 11,6. The difference of weight, between the original hundred grains of silver and the 121, was owing to 21 grains of platina, which had been drawn down in precipitation along with the silver; by an affinity for that metal. This alloy is acted upon by nitric acid, and a great part of the platina is dissolved along with the silver, nor is it very easy to separate them by the common methods.

Mercury and silver precipitated in amalgam by green sulphate.

*Exp.* 2. I dissolved one hundred grains of silver in nitric acid, and added about 1200 of mercury. I poured the mixed solution into a solution of green sulphate of iron, and obtained a very copious precipitate. When washed and dried, it weighed 939, and was a perfect amalgam, in the due proportion of mutual saturation. Its specific gravity was 13,2; but no mercury remained with it after exposure to heat.

Mercury and gold precipitated by green sulphate gave a fine blue metallic powder.

*Exp.* 3. I dissolved one hundred grains of gold in nitro-muriatic acid, and added to it about 1200 grains of mercury. Green sulphate of iron, poured into this mixed solution, caused a precipitate weighing 874. It was in the form of a fine blue powder, not resembling an amalgam, though wholly metallic. Its specific giavity I could not ascertain; but all the mercury was expelled by heat.

Experiments with recent muriate of tin, and

The re-agents which I used in the following experiments, were recent muriate of tin, and green sulphate of iron. To

 bring

bring the examples of anomalous precipitations, in mixed solutions of the metals, more clearly into view, it will be necessary to state the action of these salts, upon a solution of each metal when separate.

green sulphate of iron, as re-agents.

By recent muriate of tin we have, with a solution of gold, the well known purple of Cassius. With platina, the colour of the liquor is much heightened. With mercury, there is a total reduction. With copper, a reduction from the black oxide at 20 *per cent.* of oxigen, to the yellow oxide at 11,5 *per cent.* of oxigen. With arsenic acid, a reduction to the state of white oxide. With silver, with lead, with antimony, no reduction. Green sulphate of iron reduces none of the metallic solutions, except those of gold and silver.

Action of the recent muriate of tin on solutions of gold, platina, mercury, copper, arsenic acid, silver, lead, antimony.

of green sulphate of iron, reduction of gold and silver only.

When mixed solutions of the metals are exposed to the action of recent muriate of tin, or of green sulphate of iron, we have the following results.

*Experiments* 4, 5, 6, 7, and 8. Muriate of tin, poured into a mixed solution of gold and mercury, precipitates both metals together; and there are no traces of the purple. Mixed solutions of gold and antimony, also of gold and arsenic acid, are acted upon in the same manner. Mixed solutions of gold and copper, also of gold and lead, afford results similar to those of each metal when separate.

Exp. with mixed solutions. Muriate of tin the reagent.

*Experiments* 9, 10, 11, 12, and 13. With a solution of platina and arsenic acid, muriate of tin gives no precipitate; but the colour of the liquor is more heightened than if the platina had been alone in solution. Platina and antimony give a precipitate by this re-agent, after standing some time; but the effect is retarded by the excess of acid in the solution of antimony. Platina and copper, also platina and lead, are acted upon as the separate solutions of these metals. Platina and silver are precipitated together by green sulphate of iron.

Green sulphate

*Experiments* 14, 15, 16. Mercury and copper, mercury and lead, also mercury and arsenic, are precipitated in the metallic state by recent muriate of tin.

From these experiments it is evident,

1st. That gold has an affinity for mercury, for antimony, and for arsenic.

2d. That platina has an affinity for silver, for mercury, and for antimony; and that it is influenced by the presence of arsenic.

3d. That silver has an affinity for mercury.

Deductions of affinity between gold and mercury, antimony, arsenic; platina and silver, mercury, antimony; silver and mercury; mercury and copper, lead, arsenic.



4th. That

4th. That mercury has an affinity for copper, for lead, and for arfenic.

This feries of experiments is not intended as a fyftem of metallic affinities; but as a few facts ftated to corroborate an affertion. I am well aware that many others might be noticed; but it is not my intention to enter further into this fubject, in the prefent paper. The general importance of the principle, and the extenfive influence it is likely to have upon chemiftry, demand that it fhould be treated by multiplied refearches. The experiments that can elucidate it are of the moft delicate nature, and require peculiar care; for they do not always fucceed, unlefs performed under the moft favourable circumftances.

Mixed folutions of more than two metals are more ftriking and more complex.

When mixed folutions of three or more metals are expofed to the action of recent muriate of tin, or of green fulphate of iron, their action upon each other appears in a much more ftriking, as alfo in a much more complicated point of view.

EXPERIMENTS UPON PLATINA.

Exp. on platina.

I fhall now ftate fome experiments which I have had occafion to make upon platina, during the foregoing refearches. Very little is known concerning this metal, its oxides, or its falts; and, although I have not had occafion to extend the enquiries very far, yet my experiments may ferve to eftablifh a few points.

Platina diffolved precip. by lime; rediffolved in nitric acid; the acid expelled from the oxide by heat; oxide reduced by fimple ignition.

I diffolved a quantity of purified platina * in nitro-muriatic acid, and precipitated by lime. A great portion of platina remained in the liquor, although I had ufed an excefs of the above earth. I rediffolved the precipitate in nitric acid, and evaporated to drynefs. The refult was, a fubnitrate of platina. I then expofed the mafs, in a crucible, to a heat capable of expelling the acid altogether; and the oxide remained alone. When this was reddened, at a heat which certainly was not capable of melting filver, the oxide was reduced, and appeared with a metallic luftre. The weight of the various

* By purified platina, I have always underftood, in this Paper, platina reduced, at a gentle heat, from the falt obtained by pouring a concentrate folution of muriate of ammonia into a concentrate folution of platina.

products

products, in the above experiments, was ſuch as to give the following proportions in the oxide, and the ſubnitrate of platina.

Hence component parts of the oxide;

Yellow oxide of platina is compoſed of,

| | |
|---|---|
| Platina - - - - | 87 |
| Oxigen - - - - - | 13 |
| | 100. |

and of the ſubnitrate.

Subnitrate of platina is compoſed of,

| | |
|---|---|
| The above oxide of platina - | 89 |
| Nitric acid and water - - - | 11 |
| | 100. |

Two oxides of platina; yellow with 13 per. cent. oxygen; green with only 7 per. cent.

But, in the reduction of this oxide of platina, it became of a green colour; and remained during ſome time in that ſtate. Nitrate of platina ſometimes becomes of a pale green at the edges, when evaporated to dryneſs; and ammonia aſſumes a green colour when in holds oxide of platina in ſolution, as we have ſeen more particularly with palladium. This, therefore, is a ſecond oxide of platına. It contains but ſeven *per cent.* of oxigen.

Compon. parts of inſoluble muriate of platina;

I diſſolved a known portion of platina in nitro-muriatic acid, and expelled the nitric acid, by pouring in a ſufficient quantity of the muriatic; and then evaporated to dryneſs. By this experiment I learned, that the inſoluble muriate of platina is compoſed of,

| | |
|---|---|
| Yellow oxide of platina - - | 70 |
| Muriatic acid and water - - - | 30 |
| | 100. |

I then expelled the muriatic acid by the ſulphuric, and evaporated again to dryneſs. I found the inſoluble ſulphate of platina to be compoſed of,

and of the inſoluble ſulphate.

| | |
|---|---|
| Oxide of platina - - - - | 54,5 |
| Acid and water - - - - - | 45,5 |
| | 100,0. |

*Muriate of tin is the moſt delicate teſt of platina.*

By much the moſt delicate teſt for platina is muriate of tin. A ſolution of the former, ſo pale as hardly to be diſtinguiſhed from water, aſſumes a bright red by a ſingle drop of the recent muriatic ſolution of the latter metal. If mercury be preſent, the colour is much darker. Recent muriate of tin, poured into a ſolution of the muriate formed by the red oxide of mercury,

converts it into the muriate formed by the lefs oxigenized acids; but, fhortly after, the mercury is reduced to the metallic ftate. Hence it was, that the alloy of platina and mercury always gave a deeper coloured precipitate than platina, with muriate of tin.

The pruffiates do not precip. platina or mercury fingly; but they do jointly.

Neither platina nor mercury are precipitated by pruffic acid or by the pruffiates. But, if fulphate, nitrate, or muriate of platina be poured into pruffiate of mercury, an orange-coloured precipitate is immediately formed; and, in fome cafes, a mixed folution of platina and mercury gives a fimilar precipitate by pruffic acid alone.

Platina is fimply precip. by fulph. hidrogen.

Platina is one of the metals which are precipitated by fulphuretted hydrogen, without the neceffity of a double affinity.

*Order of affinities of platina with acids.*

The affinities of platina differ much from what is generally ftated in the tables. By the few acids I have had occafion to try, oxide of platina is attracted in the following order: fulphuric, oxalic, muriatic, phofphoric, fluoric, arfenic, tartaric, citric, benzoic, nitric, acetic, and boracic.

Remarkable theoretical obfervation.

That fulphuric acid fhould attract the oxide of platina with greater force than the muriatic, is an unanfwerable argument to an opinion which was long fupported by many philofophers, and which is not yet altogether abandoned by them. Muriatic acid has been faid to contribute to the folution of gold or platina, in nitro-muriatic acid, in the fame manner as fulphuric acid is fuppofed to promote the decompofition of water, during the folution of iron by that acid diluted. The affinity of muriatic acid for the oxide of gold or of platina, has been looked upon as the difpofing caufe that nitric acid is decompofed by thofe metals. But it is evident that fome other action takes place; for, fulphuric acid, which has a ftronger affinity for oxide of platina than muriatic acid, does not in the leaft promote the decompofition of nitric acid by gold, or by platina.

## CONCLUSION.

On the nature and ufe of general hypothefes.

The fubftance which has been treated of in this paper, muft convince us how dangerous it is to form a theory before we are provided with a fufficient number of facts, or to fubftitute the refults of a few obfervations, for the general laws of nature. If a theory is fometimes ufeful, as a ftandard to which we may refer our knowledge, it is at other times prejudicial, by creating

ing an attachment in our minds to preconceived ideas, which have been admitted without inquiring whether from truth or from convenience. We easily correct our judgment as to facts; and the evidence of experiment is equally convincing to all persons. But theories, not admitting of mathematical demonstration, and being but the interpretation of a series of facts, are the creatures of opinion, and are governed by the various impressions made upon every individual. Nature laughs at our speculations; and though from time to time we receive such warnings as should awaken us to a due sense of our limited knowledge, we are presented with an ample compensation, in the extension of our views, and a nearer approach to immutable truth.

The affinity of metals for each other, so strongly shewn in the present instance, may induce doubts to the nature of other new metals;

The affinities of metals for each other are likely to be of the most extensive influence in chemistry. They will promote scepticism with regard to future discoveries, and throw some doubts upon our present knowledge. Palladium is certainly not less different from the elements that compose it, and from all other metals, than any two can be from each other. Within the last fifteen or twenty years, several new metals and new earths have been made known to the world. The names that support these discoveries are respectable, and the experiments decisive. If we do not give our assent to them, no single proposition in chemistry can for a moment stand. But, whether all these are really simple substances, or compounds not yet resolved into their elements, is what the authors themselves cannot positively assert; nor would it in the least diminish the merit of their observations, if future experiments should prove them to have been mistaken, as to the simplicity of those substances. This remark should not be confined to later discoveries; it may as justly be applied to those earths and metals with which we have been long acquainted.

Brittle metals may perhaps be still more simplified.

With regard to the metals, we have seen how little dependance is to be placed on specific gravities. A contrary anomaly to that which operates upon platina and mercury, may take place in others; and they may become as much heavier than the mean, as the former become lighter. In this state of union, they may for a long time appear homogeneous, even by the test of chemical re-agents. One of the properties that renders metallic substances so precious is, their easy formation into such instruments as our necessities require. The fragile metals are but of secondary consequence; and, at most, serve to cont

on thofe which are ductile, fome quality which adapts them better to particular purpofes. It often happens that, by being alloyed, two ductile metals become fragile; but we have no inftance of the contrary effect in any high degree. It is therefore more to be fuppofed that we fhould look to fimplification among the fragile metals; and, even at this early period, it may not be too fpeculative to confider the metallic bodies in an order which may bring together thofe which poffefs the greateft number of fimilar characters.

Comparifons of metallic bodies with this view; As an inftance of this approximation, it may be obferved, that nickel and cobalt ftrongly participate in the properties of copper and iron. The two former metals were long regarded as mixtures; and the doubts of the ancient chemifts, who feared to pronounce as to their nature, may ftill be proved to have more foundation in truth than the affertion of the moderns, who have declared them to be fimple. Acted upon by the fame menftrua, forming infoluble compounds with the fame acids, and foluble alike in other fubftances, they have but one or two marked properties that lead us to confider them as diftinct metals. But palladium has at leaft five or fix characters, as ftrong as thofe of any metal whatfoever, that diftinguifh it, not only from its elements, but alfo from all other metals.

and earths alfo. Among the earths, this approximation is ftill more apparent. A leading character of thefe fubftances is, their tendency to enter into faline combination, in which they receive new properties, and perform new functions. If we rank them according to this general tendency, we fhall have the following order: barytes and ftrontia; lime and magnefia; glucine and alumina; zircon and filica. And, if we confider them two by two in this order, which is a natural one, we fhall bring together precifely thofe which differ by the fmalleft number of chemical characters.

Conclufion. This inveftigation might be purfued ftill further; but we muft wait the refult of experiments: a wide field is open for refearch. In the dark ages of chemiftry, the object was, to rival nature; and the fubftance which the adepts of thofe days were bufied to create, was univerfally allowed to be fimple. In a more enlightened period, we have extended our enquiries, and multiplied the number of the elements. The laft tafk will be to fimplify; and, by a clofer obfervation of nature, to learn from what a fmall ftore of primitive materials, ll that we behold and wonder at was created.

IV. *Obfervation*

## IV.

### *Obſervations on ſome remarkable Strata of Flint in a Chalk-Pit in the Iſle of Wight, in a Letter from Sir* HENRY CHARLES ENGLEFIELD, *Bart. F. R. S. to* JOHN LATHAM, *M. D. F. R. S. and L. S.* *

DEAR SIR,

AS you conſidered the ſpecimens of flint which I ſhewed you worthy of the notice of the Linnean Society, I tranſmit them to you, together with ſuch an account of the ſituation in which I found them, as may perhaps lead to a gueſs of the cauſes of their preſent very extraordinary condition, and will at leaſt ſerve as a guide to thoſe who may wiſh at a future time to inſpect the curious pit where I found them. Introductory obſervations.

Before I enter on the particular deſcription of that ſpot, I cannot help ſaying a few words on the lithology of the iſland in general, which has not that I know of, been deſcribed, as it highly deſerves, by any naturaliſt. Had I been equal to ſuch a taſk, opportunities of obſervation were wanting, and the phenomenon which I am about to deſcribe, was diſcovered by me ſo ſhort a time before I quitted the iſland, that I had not time to inſpect more than one pit, beſides that in which I obſerved it.

The Iſle of Wight, which is nearly of a rhomboidal form, lies with reſpect to its four angles, almoſt abſolutely in the four points of the compaſs. It is divided into two very nearly equal parts by a range of chalk hills, whoſe general direction is due eaſt and weſt. Theſe hills do not, however, lie in a ſtraight line, nor are they at all of equal breadth or height throughout their extent. At Bembridge, where they form the eaſtern point of the iſland, they riſe abruptly from the ſea to the height of about 400 feet; and bending a little to the northward, they continue of nearly the ſame elevation, and a very narrow breadth, till they terminate at the valley through which the Medina runs. To the weſt of the Medina the range grows conſiderably wider and is ſubdivided into ſeveral ſubordinate vallies. This additional breadth gives the ſouthern limit a great curvature to the ſouth, while the northern line remains nearly ſtraight. Their elevation increaſes much, and at Mottiſton is 700 feet. The acute and perpendicular Geological account of the Iſle of Wight.

* From the Tranſ. of the Linnean Society, vol. IV.

promontory

Geological account of the Isle of Wight.

promontory in which they terminate to the west, well known by the name of the Needles is nearly as high as Mottiston. Beside the valley of the Medina, this range is singularly interrupted by two vallies exactly similar to each other, at the two ends of the island. Brading Haven renders Yaverland at the east almost an isle, and the Yarmouth inlet cuts off the western end so nearly, that at high tides it is quite insulated at freshwater gate.

To the north of this range of chalk hills the soil is chiefly clay, with a superstratum, in many parts of gravel, the clay is intersected with many beds of stone of different qualities, and which appear to lie in great confusion. Of these some are grit with a slight admixture of calcareous matter, others have nearly equal parts of sand and lime, and others are purely calcareous. In the first which are of great hardness, very few extraneous bodies appear. In the second, are many fine impressions of shells, while the last are almost entirely composed of moulds of turbinated shells, so as to appear quite honey-combed by them. This stone is however, of great durability for the walls of Cowes Castle, which was built by Henry VIII. and is exposed to the sea air from the west and north, are as perfect as on the day in which they were built. Below all these strata of stone at east Cowes, and just above a bed of black and solid clay is a stratum of shells about two feet thick, of which a specimen accompanies this; and which is totally composed of these shells without any admixture or earth whatsoever. As the Lea makes great inroads here, vast heaps of these shells lie on the beach, and seem just washed up by the waves, instead of being torn from their bed in the cliff. They appear nearly in the same state as those on the Hampshire coast, which have long been famous among naturalists. In the bed at east Cowes, there appears, however, no variety; for I could see no species but what are here exhibited.

Whatever confusion in the strata appears to the north of the chalk range, or in that range itself, disappears to the south of it, where the strata are nearly in a horizontal position, and singularly regular and undisturbed. The sea coast from Bembridge, south to the Needles, except in the small extent of Sandown Marsh, is every where higher than the immediately contiguous land of the island, and to the south-east, rises into

a vaſt range of hills running from Dunnoſe weſt to St. Catharines. The ſubſtratum of theſe hills ſeem every where to be clay lying in ſtrata of different colour and purity. The loweſt is black and very hard; approaching to ſhale. Above this ſome ſtrata have a great mixture of ſand, and take the appearance of a ſoft ſtone, breaking into very regular cubical forms. Theſe ſtrata extend over the whole ſouthern part of the iſland, and terminate againſt the chalk range very ſuddenly. Above the clay ſtrata is a bed of ſtone in thin layers, and of very mingled materials, but in general very hard. Great quantities of chert or flint modules appear in this ſtone. The general thickneſs of the ſtratum is from 150 to 200 feet. Above this the higheſt hills of the range have a ſtratum of chalk, not pure or white as that of the chalk range properly ſo called, nor producing flint ſo black.

Geological account of the Iſle of Wight.

The height of Dunnoſe is 800 feet above low water mark, St. Catharine's hill is at leaſt 850. Of the former I had no opportunity of examining accurately the thickneſs of the ſtrata, but at St. Catharine's the ſtrata are as follow,

| | | |
|---|---|---|
| Chalk | - | 250 feet |
| Stone | - | 200 feet, or perhaps not quite ſo much |
| Clay and Sand | - | 400 feet. |
| | | 850. |

This arrangement accounts entirely for the formation of that ſingular coaſt called the Undercliff, which extends from Dunnoſe to St. Catharine's; and is compoſed of the confuſed fragments of the upper ſtratum of rock, which have given way and rolled down, as the ſubſtratum of clay has been waſhed away by the ſea. In moſt parts the proceſs ſeems nearly at a ſtand; the coaſt being now protected by the fallen rocks; but at St. Catharine's, great devaſtation is ſtill taking place. The earth-fall mentioned laſt year, was a very ſmall operation, when compared with the relics of former convulſions.

From this ſhort ſketch of the general poſition of the ſtrata in the iſland, I return to the particular ſubject of the preſent paper.

The chalk pit, which I am about to deſcribe is ſituated on the northern edge of the chalk range juſt out of the village of Cariſbrook, and about an hundred yards beyond the diviſion of the roads to Yarmouth and Shorwell. The pit is open to the

Deſcription of a chalk pit at Cariſbrook, with ſeams of flint, ſingularly broken or ſhattered.

the eaſt. The ſtrata of chalk are very regular from two to five feet in thickneſs, and divided by ſeams of flint from ſix inches to nine inches in depth: The flints are as uſual in nodules of different ſizes from the ſize of the fiſt to twice the ſize of a man's head. The whole dip northward with an inclination of at leaſt 67 degrees. Perpendicular fiſſures run through the whole from north to ſouth, the ſides of which are nearly as flat and ſmooth as a wall. As theſe fiſſures are followed with convenience in working the pit, an extenſive face was laid open when I ſaw it, and the appearance was as in the annexed ſketch. See *Plate* XI. On examining the beds of flint nearly, I was aſtoniſhed to find that every flint, though lying in its place, and retaining perfectly its original ſhape, was more or leſs burſt and ſhattered, ſome few were only ſplit into large pieces, but the greater part were broken into ſmall fragments, and ſome abſolutely reduced to impalpable powder. From one which had ſuffered the moſt, the annexed ſpecimen was taken. The powder was ſo very fine that I conceived it muſt have been mixed with chalk; but on waſhing it with diluted marine acid, I found that it was purely ſiliceous. Indeed the chalk which ſurrounds theſe flints is uncommonly ſolid, and does not exhibit cracks or marks of any violence except the great fiſſures before mentioned. A ſpecimen of the flint powder after waſhing in the acid is ſent with the other.

I muſt obſerve that I had but an imperfect opportunity of inſpecting the flints which lay at a diſtance from the fiſſure, ſuch however as I could ſee in the bed then working appeared to have been leſs ſhattered in proportion as they were more remote from the fiſſure, but all had ſuffered more or leſs.

About 200 yards below the pit, and nearer to Cariſbrook village, the road is in part cut through the chalk, and the beds of flint expoſed by that means exhibit the ſame appearances as thoſe in the pit above.

Obſervations of the flints in another pit.

The chalk pit above Shide Bridge, which is the only one I had an opportunity of examining after my diſcovery of the phœnomenon above deſcribed, preſents in ſome degree the ſame appearances, but does not afford ſo good an opportunity of viewing the ſtrata as that at Cariſbrook. The ſtrata did not appear to me to lie ſo regularly nor the flints to be diſpoſed ſo much in beds as at Cariſbrook. They were however extremely

tremely broken and ſhattered and apparently the moſt ſo where they lay moſt in ſtrata. The ſtrata had alſo a great inclination or dip to the north.

Conjecture as to the cauſe; viz. that the ſtrata at the period when their dip took place were inclined to ſlide on each other, and the flints were cruſhed by this enormous action.

Although it would be raſh to attempt to account for this very ſingular ſtate of deſtruction of the flints in the Cariſbrook pit, yet it is impoſſible not to offer ſome conjectures on the ſubject. There can be very little doubt that the ſtrata though now ſo inclined was originally formed in a horizontal poſition. When the tremendous convulſion took place which ſunk them to the ſituation in which they now appear (at which time the channel which ſeparates the Iſle of Wight from the main land was perhaps formed) the ſtrata of chalk, in the act of ſubſidence had a tendency to ſlide on each other, and this would be exerted moſt ſenſibly where from the admixture of the flints the coheſion of the parts of the chalk was the weakeſt. This motion or rather ſtrain of ſo enormous a weight might in an inſtant ſhiver the flints, though their reſiſtance ſtopped the incipient motion, for the flints though cruſhed to powder are not diſplaced, which muſt have been the caſe had the beds ſlipped ſenſibly. This conjecture is perhaps ſtrengthened by what I obſerved in a few detached nodules of flint in the chalk ſtrata which did not appear to have ſuffered as thoſe in the beds of flint have done. I may here add that it ſeemed as if in ſome places the fine powder of the flints had run down, and inveſted the nearer parts of the fiſſure with a thin coating of the agglutinated duſt; but this may poſſibly have taken place ſince the face of the fiſſure has been expoſed to the weather.

Appearances of a chalk pit near Brighthelmſtone.

Perhaps it may not be totally foreign from the preſent ſubject to mention that in a very great chalk pit at the village of Preſton, a mile north of Brighthelmſtone, in which the flints lie in a very regular and nearly horizontal ſtrata, but which has alſo vaſt perpendicular fiſſures in the chalk; the fiſſures are in many places filled to a conſiderable extent with a very thin vein of pure flint exactly as if the flint, not being quite hard when the fiſſures took place, had been ſqueezed out of the beds and run into the fiſſures as ſoft pitch would do. I do not mean at all to ſay that this was the caſe, but merely to deſcribe the appearances. In the chalk pit juſt below the church at Brighthelmſtone another ſingular appearance may be ſeen. The upper part of the chalk is in ſeparate maſſes, not properly rubble, but with all their tender angles ſharp exactly as if juſt broken

broken to pieces to put into the lime kiln, and quite clean, nearly of a fize, and almoft without any chalk powder mixed with them.

I remain, &c.

*Southampton, Jan.* 22, 1800.

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V.

*Defcription of a mercurial Air-pump, of unlimited exhaufting Power, with a Wooden Pifton, working in a Wooden Cylinder, &c. By* SIR A. N. EDELCRANTZ, *&c. &c. Communicated by the Author.*

Conftruction of an air pump in which no refidual fpace is left.

HAVING refided a long time in a place, where no mathematical inftrument maker could be had, I was obliged to contrive fome method of conftructing an air-pump, in which common workmen and common materials could be employed. Befides which, as in almoft all former conftructions of this inftrument, a fmall fpace exifts in fome part of them, where the air remains condenfed, and confequently a limit is fixed to the expanfion, I was defirous, if poffible, to remove this feemingly material, though in good pumps infinitely fmall defect. Thefe confiderations led me many years ago to a contrivance, of which the following fhort defcription may give a notion, referving a more detailed one, with a more particular account of its effects, to a future publication.

Defcription with a drawing. It operates by mercury raifed and lowered in a wooden pipe, by a plug, and the upper communication with the receiver is made through an iron cock.

A B C D, *Plate* XII. is a folid pifton of wood, cylindrical or fquare,* moving up and down in a fimilar wooden box or tube, E F G H, by means of rack-work at H. The pifton fills the box in its whole length as exactly as poffible, without touching and producing friction againft the fides. For that purpofe the pifton is guided at the lower end by an iron rod K I, fixed in the bottom of the box and entering the pifton, and at its upper part by a roller, which runs along the inner furface of the barrel. The cavity of the wooden box extends through the canal F M† to the iron cock O, and communicates at M with a per-

* I have, in the execution, preferred the fquare form, as more eafy to make exact and equal in its whole length.

† All the wooden parts are made of ftrong oak, and are covered with a good refinous varnifh, to prevent the abforption of mercury:

pendicular

pendicular glafs tube M N and the glafs fphere P. This fphere is connected with an iron cock Q R, having two oppofite perforations, one going horizontally to R, where a fmall outfide valve permits a communication with the glafs ball S Y; the other, placed perpendicularly, opens a paffage from the common receiver V T to the fphere. The inftrument being thus conftructed, the box H E F G, is filled with mercury to the dotted line *a a*, or about fix inches, and the pifton being alternately preffed down or raifed as ufual by the crank work, the effect of the machine is as follows:

Air pump operating by mercury in a wooden barrel, &c.

The cock Q R being turned as in the figure, the communication with the receiver is fhut, and that with the external air, through a fmall hole S in the glafs fphere S Y, being open, the pifton in defcending preffes upon the furface *a a*, and forces the mercury out of its place, it confequently rifes in the fmall fpace between the pifton and the wooden tube, *a a* to *b b*, and at the fame time in the glafs tube N P to a correfponding height N. The preffure being continued, the mercury ftill rifes fo as to fill the whole tube, the fphere P, and laft of all makes its way through the cock Q R and the valve R. forcing every particle of the air contained in M N P out before it. When a drop of mercury appears in Y, the pifton is moved the contrary way, and by its afcenfion the preffure on the mercury is diminifhed, which finks and evacuates the fphere P, leaving a place for the air from the receiver which is admitted by turning the cock Q R, in the other direction *. P being filled with air, the cock is turned again, the pifton again defcends, and expels the air, by raifing the mercury a fecond time, and in this manner the exhauftion may be continued at pleafure : The mechanifm and effect of this inftrument being, as I prefume, rendered clear by the preceding defcription, I fhall only add a few remarks.

The neceffary force to prefs down the pifton, being a little more than the whole weight of a column of mercury of the bafis D C, and the height of the barometer, is confiderable in the beginning of the operation, but diminifhes as in other air-pumps, when the air in the receiver becomes more rarefied; when the external atmofphere acts more powerfully in fupport

* The alternate turnings of the cock may eafily be performed by a fimple appendage connected with the motion of the pifton.

Air pump operating by mercury in a wooden barrel, &c.

of the column M N, which in every fucceffive ftroke becomes higher, till the exhauftion is nearly compleat, when its height will be that of the barometer.

In order to equalize the preffing force more nearly during the whole operation, the upper end of the pifton may be loaded with a weight equal to half the original preffure.

The mercury being a fluid, fills every angular fpace as paffes, till it opens its way through the valve R; confequently no particle of air can be left in a condenfed ftate to be more rarified or diminifhed by the next ftroke of the pifton. By this means the common defect of air-pumps feems to be remedied, and the exhauftion unlimited. Befides, as neither moifture or oil are introduced in this pump, the vapours arifing from them in great exhauftions are prevented.

The quantity of mercury required, when the diameter of the wooden box and of the fphere P are four inches, will be about fixty pounds, which if taken out by the cock O, may ferve alfo for another ufeful apparatus in the fame laboratory: the mercurial air holder.

Several projects for mercurial air-pumps have been publifhed before, although I think none executed; but a comparifon will, as I fuppofe, eafily fhew this conftruction to be quite different from any of them.

For greater folidity, I have in the execution made three-fourths of the tube M P of iron. The fphere P is likewife fixed and faftened by means not fhewn in the figure, but which may without difficulty be apprehended.

Excepting the cock Q R, which requires fome precifion of workmanfhip, all the parts of the machine may be executed by common workmen, fuch as carpenters, fmiths, &c. who are every where to be found.

VI. *On*

## VI.

*On the Combinations for supplying Worm Tubs with Cold Water, by the Syphon, with an easy Method of securing the Joints of such Hydraulic Apparatus against the Admission of the External Air. In a Letter from Mr.* WM. CLOSE.

To Mr. NICHOLSON.

SIR,

*Dalton, Feb.* 13, 1804.

THE combinations which have lately appeared in your Journal, for supplying worm tubs, condensers, &c. with cold water, have recalled my attention to certain experiments which I formerly made with the syphon; and I am of opinion that the method proposed will succeed very well upon the simple construction of the apparatus delineated in *Plate* IV. *Vol.* VI. of your Journal, if all the parts are made air-tight, and a sufficient quantity of water be provided to keep the syphon in constant action, under an impending column of two feet. For the air disengaged from the water in the worm-tub will rise into the bend of the syphon at B. *Plate* IV. *Vol.* VI. and be carried down the descending column and emitted at C.

Reasons why the syphon principle is applicable to the supply of worm tubs, &c.

The worm tub may be effectually secured against the admission of the external air through the joinings of the staves, by being placed in a larger tub filled with water; no air then can possibly enter the interior tub so long as it is covered, and the workmanship must be very bad, if the syphon should draw the water out of the exterior vessel, so fast as to produce any inconvenience in replenishing it as often as it subsides.

Easy method of excluding the external air, by surrounding the worm tub with water.

The worm must be made air-tight before it is fixed in its place. If the supply of cold water at A should be interrupted, the fluid in A will subside to a level with the surface of C, and, if much air be disengaged during this suspension, and while the water is impending in the apparatus, the syphon will probably not resume its operation: In this case the two apertures must be closed, and the apparatus filled with water.

To set the machine at work when stopped.

You will remember the principles on which I planned the construction of a machine for raising water by the syphon. I have the complete model of such a machine: it has four light, self-acting valves, and when these are not prevented from closing, by impurities in the water, &c. it performs very well.

Account of the effect of a syphon engine formerly described.

The

The ſyphon is not quite two inches wide; it riſes about 31 inches above the reſervoir in which it is placed, and the deſcending column is ſix inches longer than the aſcending one. The machine is conſtructed with all the parts repreſented in *Plate* III. *Vol.* I. but under a different arrangement. It delivers about three pints of water every minute, at 31 inches above the reſervoir: the alternations are repeated every five ſeconds.

Its power in carrying down air.

In the operation of this machine, a bulk of air equal to three pints of water is carried down the deſcending column every minute, when the fall is only ſix inches; but I am certain from experiment that the ſyphon would clear itſelf of a much greater bulk in the ſame time.

Hence the worm tub ſyphon will be effectual in carrying off the air extricated from the water.

In an experiment on the 29th of May 1802, with a ſyphon of the above dimenſions, and a fall of ſix inches, it appeared by a graduated bottle which received the raiſed water, that the ſyphon would take a quantity of air equal in bulk to four ounces of water at leaſt, every three ſeconds. I think, therefore, that the ſmall quantity of air which will be diſengaged from the water in the worm tub at a low temperature, by the operation of the ſyphon, with a fall of two or three feet, will be carried off as faſt as it is produced, unleſs the pipe be very narrow in proportion to the tub,

I am, Sir,

Your's reſpectfully,

WILLIAM CLOSE.

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## VII.

*An Anſwer to Dr.* WOLLASTON'S *Statement, "Of an Improvement in the Form of Spectacle-Glaſſes." By Mr.* WILLIAM JONES, *F. Am. P. S. Optician, Holborn.*

The ſpectacle glaſſes of Dr. Woollaſton cenſured.

OBSERVING in your Journal for laſt Month, that Dr. W. H. Wollaſton, by a paper inſerted therein, is attempting to introduce into the conſtruction of ſpectacles, the well known and obſolete form of lens called *a meniſcus*; I beg leave, Sir, to offer through the channel of your ſcientific and impartial record, a few obſervations on his arguments; and my reaſons why I do not conſider the contrivance as entitled to any claim either of novelty or improvement.

When

When a printed book, or other object, is received through a convex ſpectacle-glaſs, or other lens of a ſhort focal diſtance, ſuch as from ſeven inches down to four inches or leſs, the indiſtinctneſs obſerved of the ſurrounding parts, when the central appear diſtinct, ariſes from the ſpherical figure of the lens, and is by Opticians called the longitudinal aberration of the lens. There is another kind of aberration connected with this lens, called the lateral aberration, which is occaſioned by the priſmatic form of the lens, producing different refrangibility of the rays of light, and blending the priſmatic colours with the appearance of the object. It is the longitudinal aberration only that I have now occaſion to conſider. This aberration in lenſes of the ſame foci, increaſes with the diameter and thickneſs; and of the ſame diameter is in the inverſe proportion to the foci. Aberration of light refracted by lenſes.

The rays iſſuing from diſtant objects, are more parallel to each other when incident upon the lens, than thoſe from proximate ones; therefore the aberration will be leſs.

Hence, it may be inferred, that when ſpectacle-glaſſes have been made larger in diameter than is ſufficient to obſerve through, the angular extent of objects, they refract ſuperfluous light, and have very properly been ſomewhat reduced in diameter, ſo that a perſon might uſe them without much inclining the axis of his eyes, or finding it inconvenient or unnatural to move his head a little. For the aberration is thus diminiſhed, and conſequently the objection in a great degree removed, except when the glaſſes are of very ſhort foci. Contracting the diameter of ſpectacle lenſes approved.

In concave glaſſes, the aberration or indiſtinctneſs is of a ſimilar nature; the defect of theſe being from the imperfect divergence of the rays, inſtead of the imperfect convergence in convex glaſſes. Concaves.

Spectacle-glaſſes are now generally made of the double concave, and double convex forms or nearly ſo; for a little alteration of figure does not affect the general appearance of objects viewed through them. It is in ſcience, as in other caſes, that general utility does not always depend on trifling alterations. Spectacles are recorded to have been invented about the year 1300, and from much reading and many years experience in this ſmall, but invaluable article, I really do not know, that during the elapſed time, any optical inſtrument of any kind whatſoever has undergone more innovation, and Remarks on ſpectacles and ſome of the improvements made in them.

attempts at improvement. Of many within my knowledge, I ſhall only ſelect the following as entitled to any degree of commendation. *Ayſcough's* crown glaſs ſpectacles. The biſected glaſſes of *Dr. Franklin*. *Ribright's* double glaſſes. The viſual glaſſes of that learned Optician *Mr. Martin*. The ſquare convex form by *Stour*. The patent combined glaſſes of Meſſrs. Watkins and Smith, injudiciouſly called achromatic, conſiſting of a convex lens, combined with a *meniſcus* or concave convex lens. In the various mountings of the frames there is a ſtill greater variety.

The original form moſt approved.

Notwithſtanding theſe contrivances, univerſal experience has cauſed the original and ſimple form of glaſſes to ſuperſede them; and it affords an indubitable proof, that it is the beſt and moſt convenient that can be deviſed, when clear glaſs, accurate tools, and good workmanſhip are uſed.

Theorem of Huygens conſidered as univerſally applicable.

The theorem given by *Huygens*, and demonſtrated by many other ſubſequent writers on optics, proving that a convex lens having its radii of curvature in the proportion of one to ſix, has leſs aberration than any other form of lens, when the greateſt convexity is towards the object; and the ſame for the concave lens; muſt hold true for any uſe whatſoever, for which ſuch a formed lens may be required.

Eye glaſſes of ſpectacles.

It does not appear to have occurred to Dr. W. that the eye glaſſes uſed to magnify the images formed by the object glaſſes in teleſcopes are of the beſt form, when with the curves of the proportion above mentioned. In the eye pieces of the beſt achromatic teleſcopes, they are always applied, and in high powers, the image frequently ſubtends an angle from the centre of the eye glaſs of ſixty degrees or more. I have never ſeen any correct dioptrical theorem, that tended to prove that a meniſcus, ſingly, or combined, will anſwer ſo perfectly the ſame purpoſe.

Remarks on lenſes; to aſcertain the value of Dr. Wollaſton's patent ſpectacles.

The ordinary purpoſes of viſion, are very well anſwered by the common glaſſes, under an angle as large as eighty or ninety degrees, and the beſt artiſts, or draughtſmen allow, that 60° is as much as a fixed poſition of the eye ought in perſpective to embrace, to convey a juſt repreſentation on the optic nerve.

To perſons, the humours of whoſe eyes are ſo decayed as to be deficient in their original refractive power, glaſſes of ſhort

foci

Remarks on lenſes; to aſcertain the value of Dr. Wollaſton's patent ſpectacles.

foci will, to them, render the extreme parts of objects ſomewhat confuſed; but in a much leſs degree than to perſons, with perfect eyes, or undecayed humours.

In teleſcopes and microſcopes, the aberration is uſually cut off by the inſertion of circular apertures or ſtops; but in ſpectacles this is not eſſentially neceſſary, nor does the want of them or the figure of the glaſſes, prove that they are conſtitutionally bad and prejudicial.

The obſervation of Dr. W. that only a portion of the glaſs a little larger than the pupil of the eye, is employed at once, is only juſt inaſmuch as it relates to the mind, being intent on a point of an object ; but not ſo in regard to a general view. For the refractive power of the lens does moſt admirably collect all the infinite number of pencils of rays, or cones, into one concurrence at the pupil of the eye; where they croſs or interſect each other: yet, ſuch is the exquiſite ſubtilty of light, that no confuſion or irritation takes place. Man is thus bleſſed by aſſiſted viſion, as he is in vitality by the reſpiration of air.

Dr. W's inferring the form of a meniſcus from the ſhape of a globe, is manifeſtly erroneous; and in reſpect to ſpectacles inappropriate. A glaſs globe or ſphere without any ſenſible thickneſs, to an eye exactly placed in its centre, admits all the incident rays to paſs throughout it unrefracted. If the eye deviates from the centre, a refraction will take place, and that in proportion to the thickneſs of the ſphere. Rays of incidence paſs perfectly unrefracted through truly ground plane, or parallel glaſs, to an eye before it; and let the axis of the eye, be ever ſo much incloſed, unleſs the glaſs be very thick, the object will ſtill appear perfect, and no refraction of the incident rays will be obſerved. It is obvious, therefore, the nearer a lens approaches to the figure of a plane, the more perfect it muſt be.

The figure of a meniſcus, which Dr. W. wiſhes to adopt, is as different from a ſphere, as a plane. Its figure is compoſed of two portions of ſpheres, of different radii. When with a poſitive focus it is mathematically demonſtrable, that it has entirely the properties of a convex lens, and with a negative focus the properties of a concave one. When the radius of the exterior curve, is leſs than that of the interior it is a convex ſort of lens, and magnifies; but, when the radius of the interior curve is leſs than that of the other, it is

 a con-

Remarks on lenses; to ascertain the valve of Dr. Wollaston's patent spectacles.

a concave lens, and diminishes. It has also been demonstrated, that the nearer the form of a meniscus approaches to that of a plano convex or concave, the more perfect it will be, and produce less aberration.

I shall dispense here with the proofs by algebraical and analytical formulæ, as any qualified reader will find them in the optical works of *Huygens, Molineux, Euler, D'Alembert, Smith, Emerson and Martin;* and many others.

The rays of light issuing from a near object to a spectacle glass before the eye, are in diverging pencils or cones, and the meniscus form of glass of any certain positive focus, will refract them towards a state of parallelism into the eye, necessary to produce distinct vision, in decayed sight, precisely in the same manner as a double convex or planó convex glass of the same focus would do. A meniscus with a negative focus, acts no ways different from the double or plano concave glasses; the rays of light being divergent somewhat to counteract the effects of too great a convexity in the humours of the eye of a short sighted person. Perhaps, it is hardly necessary to observe, that imperfect vision in the optical sense, consists in the long sighted eye, in the rays of light not being sufficiently converged by its humours, to meet on the retina of the eye, but falling beyond it; and in a short sighted eye by the rays converging too much, so as to meet before they reach the retina.

Varying the geometrical figure of a lens, does not constitute any new optical principle, for any of the common species of lenses, may be cut into the form of a square, a triangle, an oval, &c. all figuratively various, but consisting only of one optical principle.

The use of the meniscus has been abandoned by Opticians, by its containing in comparison with other lenses, the greatest spherical surface, and consequently producing the greatest aberration. Reducing the curvatures of the meniscus elongates the focus, in the same manner as in other lenses, and therefore reduces the aberration. Hence in spectacle-glasses that are not of short foci, no preceptible difference will be found to persons unacquainted with optical experiments.

There are various practical methods that will point out to persons the aberration of lenses here spoken of, and that the minifcus

menifcus caufes the greateft of any of the other forms of lenfes; but, the following, I would recommend as the moft eafy and illuftrative.

Remarks on lenfes; to afcertain the valve of Dr. Wollafton's patent fpectacles.

Take a menifcus lens about the fize of a fpectacle-glafs, and with four inches pofitive focus, and take alfo a plano convex or a double convex, of the fame diameter and focus, in a room with one lighted candle, at a diftance by night, hold the convex glafs near to the white wall or wainfcot fide of the room, between it and the candle; move the lens backwards and forwards till a clear image of the candle is formed, which will be a diftinct inverted image of it: do the fame with a menifcus, and there will be this difference obferved by the menifcus, that incircling the vivid image of the flame, there will be a faint white light, which is the circle of the aberration; and, evidently fhews, that it is the worft form of the two for a fpectacle-glafs, or any other purpofe.

Two convex double glaffes placed together in one cell, contain lefs aberration than one glafs of the fame diameter and focus; and two plano convex-glaffes with their convex fides placed together in one cell, give ftill lefs aberration. It is lofs of light only that can be objected to. They are too weighty to be adopted in fpectacles, but in eye pieces of large telefcopes for viewing celeftial objects, they have been ufed with great advantage. To engravers, miniature painters, and other artifts, they are moft ufeful, as by fhort foci and large apertures, they give them the moft diftinct view of a large furface placed before them.

For the fatisfaction of an intelligent perfon, who may be difpofed to have an ocular proof of the properties of glaffes as herein advanced, I have conftructed a frame, containing a double convex, plano convex, a menifcus, two plano convexes, with their convex fides to each other, all of the fame diameter, and the foci about four inches, and by which may be feen that the greateft peripheretical indiftinctnefs is with the menifcus glafs. This apparatus will be fhewn by application at our manufactory in Holborn.

The menifcus as a figure for a fpectacle glafs I confider very objectionable. To afford a large field of view, its diameter muft be confiderable; which for a fhort focus will increafe thicknefs, protuberance outwards, and weight; and, in concave glaffes occafion the frames to be made thicker; the glaffes will

will be more liable to be ſcratched and broken than thoſe of the common form, and when the frames are metallic, more likely to increaſe than diminiſh that indelible mark made on the noſe by the weight of the frame, ſo frequently complained of by perſons who wear ſpectacles almoſt conſtantly. A great deal of ſuperfluous light alſo paſſing through the glaſſes, muſt be evidently prejudicial; and, it appears to me that the concave figure of the inner ſide of the meniſcus will act as a powerful reflector to condenſe the rays of light and heat upon the eyes, and ultimately prove thereby of ſerious injury.

I have in my poſſeſſion a meniſcus ſpectacle glaſs, taken from a ſpectacle frame, which I can prove to have been made many years ago; and, finally, as this form is neither new in principle, or in practice, I am at a loſs to know upon what ſort of diſcovery his Majeſty's letters patent have been ſolicited.

I am,

Sir,

Your's, &c.

W. JONES.

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## VIII.

*An Account of an Occultation of b Nebulae Sagittarii, by the Planet Mars, on the 17th of April 1796, obſerved by Sir* HENRY ENGLEFIELD, *Bart. F. R. S. In a Letter addreſſed to the Rev.* NEVIL MASKELYNE, *D. D. F. R. S. and Aſtronomer Royal.* *

DEAR SIR,

Occultation of a fixed ſtar by the planet Mars.

I BEG leave to communicate to you an obſervation, which I had the good fortune to make, of an occultation of a fixed ſtar by the planet Mars; and which, as far as I can learn, was ſeen by nobody in this country but myſelf and Lady Buck, at whoſe houſe at Peterſham I made the obſervation, and to whom I ſhowed the planet when nearly approaching to conjunction with the ſtar.

Theſe obſervations are very rare.

Obſervations of this kind may be ranked among the rareſt phenomena in aſtronomy. Mr. De La Lande records but four

* From the Journals of the Royal Inſtitution, No. 16, p. 38.

four ſince the invention of teleſcopes. Of theſe, one only is by Mars, and that ſo long ago as about the year 1630, by Gaſſendi. The curious and very important conjunction of Mars with pſi aquarii, in October 1672, whence Caſſini had hoped to deduce, with great accuracy, the parallax of Mars and the Sun, was miſſed by bad weather at the moment of the occultation. Since that time, I do not know that any obſervation of this kind has been made.

Account of the obſervation. The night of April 17, 1796, being very clear, I was led by mere accident to direct my teleſcope to Mars, about two o'clock in the morning; and I ſaw with equal pleaſure and ſurpriſe, a ſtar, about three of his diameters to the eaſt of him, and in a line perpendicular to his horns, he being then gibbous. It was evident that an occultation would take place, and I prepared every thing for the obſervation.

Inſtruments. The teleſcope I uſed is an achromatic of Ramſden, the aperture 2.75, with a ſingle eye-glaſs, magnifying 100 times. The clock is a very good regulator, with a wooden pendulum, and to be depended on to leſs than 2 ſeconds in the 24 hours. The obſervations for the time were made with an excellent ſix inch Hadley's ſextant of Ramſden's, and an artificial horizon of mercury.

Place of the obſ. The poſition of Peterſham, as deduced by a ſurvey made by me ſome years ſince, from the Royal Obſervatory at Kew, which was one of the points ſettled by General Roy in his great ſurvey, is 1′ 12″ in time weſt of the Obſervatory at Greenwich in longitude; and its latitude by the ſame ſurvey 51° 26′ 34″.

Collateral circumſtances. To return to the obſervation. The ſky, during the whole time, was perfectly free from clouds, but was remarkably full of vapours, which at times produced a greater undulation in the limb of Mars, than I almoſt ever ſaw. The wind was in the eaſt, and probably the columns of warm air blown from London, mixed very irregularly with the atmoſphere of Peterſham. This ſtate of the air, and the low altitude of Mars, whoſe ſouthern declination was 23°½, rendered it impoſſible to diſtinguiſh with certainty the ſtar, from the protuberances in the waving limb of Mars at the moment of immerſion, though the brilliant and ſilvery light of the ſtar contraſted ſtrongly with the dull red hue of the planet. The obſervation of the immerſion of the ſtar is therefore uncertain, to near a minute of

of time. At the emerſion the ſtate of the air was rather more favourable than at the immerſion; and as the ſtar emerged from behind the dark limb of Mars at a ſenſible diſtance from his enlightened edge, this obſervation is much more certain than that of the immerſion; and is probably true, to five, or ten ſeconds at moſt.

Effect of the atmoſphere of Mars, probably.

The ſtar at its firſt emerſion, certainly appeared much fainter than it did ſoon afterwards; but I cannot pretend to decide, that this apparent diminution of its light was owing to any other cauſe than its great proximity to the ſuperior light of the planet. Yet it was at firſt ſo faint as to be very doubtfully viſible, and in leſs than a minute was very conſiderably brighter. As in this interval of time, its diſtance from the planet was not very much increaſed, it is probable that the atmoſphere of Mars might have been the cauſe, at leaſt in ſome degree, of this appearance.

Occultation of a fixed ſtar by the planet Mars.

*Occultation of b ♐ by the Planet Mars, April* 17, 1796, *at Peterſham.*

Immerſion of b ♐ per clock . . . . . $15^h\ 4'\ 48''$
Uncertain to nearly one minute.
Emerſion of b ♐ per clock . . . . . $15^h\ 24'\ 28''$
Certain to five, or ten ſeconds at moſt.

The ſtar paſſed to the north of the centre of Mars. The immerſion was about one third, or at moſt two fifths of his ſemidiameter north of the centre. The emerſion uot quite half the ſemidiameter north of the centre, by eſtimation.

The clock, at the time of the occultation,
was faſt on mean time . . . . . . $0^h\ 4'\ 21\frac{1}{2}''$
The immerſion was therefore at . . . $15^h\ 0'\ 26\frac{1}{2}''$
The emerſion at . . . . . . . . . . $15^h\ 20'\ 6\frac{1}{2}''$
Mean time at Peterſham.

And, Greenwich being $1'\ 12''$ in time eaſt of Peterſham, the obſervations reduced to the meridian of Greenwich are,

| | Mean Time. | Apparent Time. |
|---|---|---|
| Immerſion . . . | $15^h\ 1'\ 31\frac{1}{2}''$ | $15^h\ 2'\ 28\frac{1}{2}''$ |
| Emerſion . . . | $15^h\ 21'\ 18\frac{1}{2}''$ | $15^h\ 22'\ 8\frac{1}{2}''$ |

Although this obſervation is ſubject, from the circumſtances ſtated before, to an uncertainty of half a minute in time, as to the middle of the occultation; yet, as the geocentric mo-

tion

tion of Mars, was at this time not quite two feconds of a degree in three minutes, his place (that of the ftar being accurately known), may certainly be determined by this obfervation within a fingle fecond; with a view, therefore, of afcertaining the accuracy of the tables of Mars, given in the laft edition of De la Lande's Aftronomy, the longitude and latitude of Mars were computed with care as follow: Occultation of a fixed ftar by the planet Mars.

1796, April $17^d\ 15^h\ 12'\ 18''$ apparent time at Greenwich.

| | Geoc. Long. | Geoc. Lat. Auft. |
|---|---|---|
| Mars | $8^s\ 27^\circ\ 7'\ 37''$ | $0^\circ\ 20'\ 41''\ 40'''$ uncorrected, |

for aberration or parallax. And April, $18^d\ 15^h\ 12'\ 18''$ his place was $8^s\ 27^\circ\ 23'\ 2''$ $0^\circ\ 22'\ 58''\ 18'''$.

His daily motion in longitude, was therefore $0^\circ\ 15'\ 25''$, and in latitude $0^\circ\ 2'\ 16''\ 38'''$ increafing.

His aberration in longitude at the time of obfervation, was $-\ 3''.97$, and his horizontal parallax, that of the Sun being taken at $8''.72$, was $11''.712$.

The parallax in longitude and latitude, computed by the tables of the Nonagefimal for Greenwich, given in the *Connaiffance des Tems* for 1775 is, Par. Long. $+\ 1''.772$, and Par. Lat. $+\ 11''.2$.

The apparent place of Mars at the time of the occultation was, therefore, by thefe tables,

| Geoc. Long. | Geoc. Lat. Auft. |
|---|---|
| $8^s\ 27^\circ\ 7'\ 34''.8$ | $0^\circ\ 20'\ 52''.9$. |

But the place of the ftar, as fettled at Greenwich, was at the time of occultation,

| Long. | Lat. Auft. |
|---|---|
| $8^s\ 27^\circ\ 6'\ 19''$ | $0^\circ\ 20'\ 41''$ |

And as the centre of the planet paffed about two fifths of his femidiameter fouth of the ftar, and the apparent diameter of Mars at that time was very nearly fourteen feconds and an half; the difference of latitude between the ftar and Mars, may be eftimated at three feconds, without poffible error of above half a fecond.

The apparent place then of Mars, as found by obfervation, was,

| Geoc. Long. | Geoc. Lat. Auft. |
|---|---|
| $8^s\ 27^\circ\ 6'\ 19''$ | $0^\circ\ 20'\ 41''$ |

and the error of the tables is in longitude $+\ 1'\ 15''.8$, and in latitude $+\ 12''$.

Had

Occultation of a fixed ſtar by the planet Mars.

Had this obſervation been made under favourable circumſtances, the diameter of Mars might probably have been deduced from it with a great degree of accuracy; although ſuch determination would always have been ſubject to uncertainty, from the effects of the refraction of his atmoſphere, which it is impoſſible to eſtimate, but which ſeems, from ſome circumſtances, to be very conſiderable.

In the preſent obſervation, two other cauſes of uncertainty exiſt. The firſt is, the doubt of the duration of the occultation; the ſecond, the poſſibility of miſtake in the eſtimation of the diſtance of the ſtar from the centre of Mars.

The firſt of theſe cauſes cannot induce an error of more than two thirds of a ſecond in the eſtimate of his diameter. Probably the error is not more than half that quantity.

The probable error ariſing from the ſecond cauſe, may amount to about the ſame quantity as the former. Of this I formed as near a judgment as I could, by drawing Figure 2, *Plate XII.* and placing the path of the ſtar at its utmoſt limits of diſtance from the centre by my eye. If the effects of both theſe cauſes lie the ſame way, an error of a ſecond and an half in the determination of the diameter of Mars, may poſſibly ariſe; but it is highly probable, that the error is very much leſs than that quantity, putting the effects of refraction out of the queſtion.

Having, therefore, to the beſt of my power, compared all the circumſtances of the obſervation, and the effects of the above cauſes; I conſider the equatorial diameter of Mars, as reſulting from it, to be at that time 14″.5: and as the diſtance of Mars from the Earth was then 0.71456, the Sun's mean diſtance being 1, the diameter of Mars at the mean diſtance of the Sun, would be 10″.8. Dr. Herſchel, in his laſt paper on Mars, makes it 9″.8.

As all the cauſes of error in my obſervation, tend rather to diminiſh than increaſe the ſize of the planet, I am ſurpriſed that I ſhould have exceeded his eſtimate; which is, however, leſs than that of former aſtronomers.

This, Sir, is all that has occurred to me on the ſubject of this very uncommon obſervation.

You will perceive, that I have not, in my eſtimations of the diameter of Mars, or of the diſtances of the ſtar from his limb, made any mention of the ſpheroidical figure of the planet. The differences in the reſults would be perfectly inſenſible,

fible, if that element had been confidered; and as I never have been able to perceive the difference of his diameters, I cannot but fufpect it to be lefs than Dr. Herfchel fuppofes.

Occultation of a fixed ftar by the planet Mars.

I am,

Dear Sir,

Your obliged and faithful,

*May* 23, 1797. H. C. ENGLEFIELD.

P. S. In the *Connaiffance des Tems* for the year VI. page 157, and year VII. page 428, are accounts of an obfervation of this occultation, made at Aubenas, or Viviers (for it is not certain at which of thefe places) by Mr. Flaugergue, member of the National Inftitute. It is not clear from thefe accounts that he actually faw the emerfion: at $15^h\,58'\,25''$, apparent time, he faw the ftar, diftant from the limb of the planet only by an interval equal to the fuppofed breadth of the dark part of the difc of Mars, and it was extremely faint, but perfectly white. Its pofition was about 20° to the weft of the vertical, and above the horizontal diameter. Mr. Flaugergue fuppofes that the ftar paffed within a fecond of the northern limb of Mars, and muft have been hidden ten minutes and an half.

As the place of obfervation was in a lower latitude than London, the obferver there muft have feen the ftar pafs nearer the centre of Mars than I did, the effect of parallax being lefs, and the occultation muft in confequence have lafted longer: but my obfervation proves, beyond a doubt, that the ftar paffed much nearer the centre than Mr. Flaugergue fuppofes, as I loft fight of the ftar at leaft nineteen minutes and an half.

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## IX.

*Letter from Mr.* EZEKIEL WALKER, *containing a confiderable Improvement in Time-Pieces by Mr.* BARRAUD, *with Remarks.*

To Mr. NICHOLSON,

SIR,

THE method of determining the longitude by time-keepers, poffeffes fome advantages fuperior to any other that has yet been propofed. In the firft place, this method is attended with

The determination of longitude by a time-keeper is more eafy and can be oftener

made than by other methods. with very little trouble; and, fecondly, the longitude can be found oftener by a time-keeper than by any other means. I have not met with a more ftriking proof of this pofition, than in the voyage of La Peroufe round the world in the fhip La Bouffole.

Remarkable cafe in proof. Between the 1ft of Auguft 1785, and the 8th of September 1787, this celebrated navigator took only 72 lunar obfervations; but he took 393 obfervations on the longitude by his time-keeper No. 19. This is an unequivocal proof of the benefits that navigation derives from this mode of finding the longitude.

But time-keepers are very expenfive, and liable to irregularity. It is to be regretted, however, that time-keepers are expenfive and liable to ftop, or go irregularly. Were it not for thefe inconveniences, no other method of finding the longitude need be fought after. But a difcovery was made by Mr. Barraud, about the clofe of the laft century, on the effects of oil on time-keepers, which not only reduces them in price, but contributes to their performing with greater precifion.

Difcovery of Mr. Barraud, by which time-keepers are rendered more perfect, and cheaper. Mr. Barraud had frequently communicated to me his improvements in chronometry, but this difcovery on the effects of oil, appeared a matter of fo much importance, that I wrote to him, requefting his leave to publifh it, and the following extract is taken from his obliging anfwer.

To Mr. WALKER.

DEAR SIR, *London, Jan.* 28, 1804.

Letter from Mr. Barraud. I AM much pleafed to find that it is your intention to favour the public with your obfervations on chronometry, and fhall derive fatisfaction in contributing my mite to fo defirable an end: you are therefore welcome to ufe fuch information as my experience of facts enables me to furnifh.

Jewelling the holes of time-keepers is injurious; The ftate of my regulator I have already defcribed [in a former letter], from which I infer, that fo far from jewelled holes being advantageous in clock-work, they are abfolutely injurious. That they are equally fo in chronometers, I have had abundant experience, having found, almoft without exception, in chronometers coming off a long voyage, the oil in a much worfe condition in the jewelled holes (particularly in thofe where the friction was confiderable) than in the brafs ones.

the oil in fuch holes being fooner vitiated than in brafs holes. I have therefore been induced, in every inftance, to reject jewelled holes, and introduce thofe of brafs, and the

alteration

alteration has been conftantly favourable to the performance of the time-keeper. I fhould be happy in having a communication with you *viva voce* on this theme, &c. &c.

I am, &c.

P. P. BARRAUD.

That Mr. Barraud has not made this alteration in his chronometers, in a hafty manner, will appear from the following extract taken from another of his letters.

To Mr. WALKER.

DEAR SIR, *London, July* 17, 1800.

SINCE we parted, I have found additional reafons to believe that jewelled holes (where friction is great) are injurious. A box time-keeper which I have recently taken to pieces, on its return from a long voyage, had the oil in the brafs holes in a much purer ftate than in the jewelled ones. In the former it ftill remained in a ftate favourable for action, but in the latter, the pivots were fo fixed by the tenacity of the oil, as to require force to extricate them; the fteel was alfo deeply ftained, and had parted with all its luftre, &c. &c.

Inftance of a time-keeper, in which the oil was much more vitiated in the jewelled than in the brafs holes.

I am, &c.

P. P. BARRAUD.

From thefe and many other obfervations made by Mr. Barraud upon the effects of oil on time-keepers, it appears that fmall particles of fteel are worn off by friction in the jewelled holes, and mixing with the oil, form a glutinous fubftance that caufes the time-keeper to go irregularly.

Moft probably by abrafion of the fteel pivots.

*On the Action of Cold on Oils.*

IT is a known property of fome oils, that they freeze much fooner than water. The oil of olives freezes at $42\frac{1}{2}°$ on Fahrenheit's fcale, confequently the pureft oil of this kind will lofe its fluidity fooner than that which contains fome aqueous particles. Hence we may conclude, that watchmakers ought to make choice of that oil which freezes with the leaft degree of cold; and as cold has no power to decompofe olive oil, it need not be rejected on account of its having affumed the concrete form.

Olive oil freezes at $42\frac{1}{2}$ degrees.

In

In confequence of this oil freezing much fooner than water, the following queries feem to claim our attention:

Hence it may perhaps be neceffary to adjuft and keep time-keepers at temperatures between 43° and 100°.

*Query* 1. Will a time-keeper go at the fame rate when the oil is frozen, that it did when the oil was in a fluid ftate?

If this query be anfwered in the negative, *Query* 2. Would it not then be improper to adjuft the compenfation balance in frofty weather?

*Query* 3. Would it not be better to adjuft the compenfation for the effects of heat and cold at 43° for the greateft degree of cold, and at 96 or 100° for the other extreme?

A chronometer adjufted in this manner, fhould never be expofed to a greater degree of cold than 43. This may be eafily done with pocket chronometers, but to keep the oil from freezing in box time-keepers, in cold climates, more care may be requifite.

Oil has other properties which ought to be carefully examined before it be applied to time-keepers, but this is an inquiry which muft be left until fome future opportunity.

I am, Sir,

With much refpect,

Your humble fervant,

E. WALKER.

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## X.

*Letter from* C. WILKINSON, *Efq. containing Facts upon which Deductions are made to fhew the Law of Galvanifm in burning the Metals, according to the Difpofition of equal Surfaces of charged Metallic Plate.*

To Mr. NICHOLSON,

SIR,

IF the following obfervations fhould be deemed worthy of infertion in your valuable Journal, I fhall take the liberty of troubling you with fome further remarks hereafter.

I am, Sir,

Yours refpectfully,

*Soho Square.* C. WILKINSON.

When the French philofophers had afcertained that a feries of galvanic plates produced effects on animal fubftances in proportion to the number of plates employed, without any regard to the furface of each plate, they concluded from their experiments, that the effects of a galvanic battery on metallic fubftances are in proportion to the furfaces of the plates employed. I have lately been engaged in experiments with the moft extenfive galvanic apparatus hitherto conftructed, from which fome circumftances have occurred, in fome refpects militating with the deductions of the French philofophers. Introduction.

A galvanic trough containing one hundred fquare plates of four inches in the fide, each plate formed of a plate of zinc and copper foldered together; when charged with a folution of nitrous acid and water in the proportion of about 25 parts water to one of acid, exhibited a power capable of igniting half an inch of fteel wire of about one feventieth of an inch in diameter. One hundred galvanic plates of four inches fquare burned half an inch of wire.

When two fuch troughs were combined endwife, the power was doubled, and when four were thus arranged, the quantity of wire ignited was quadrupled; hence I afcertained in a very extenfive arrangement, that the power is invariably in proportion to the number of plates employed. Other troughs added endwife burned more wire in proportion to the number of plates.

A galvanic trough confifting of fifty fquare plates of eight inches each in the fide was charged with a fimilar prepared folution, and this arrangement I found capable of igniting fixteen inches of the fame wire as was employed in the former experiment. A trough of larger plates had much more power,

When two, three, and four troughs of the fame fize were combined, the lengths of wire ignited proved to be in proportion to the number of plates employed; fo that two hundred eight inch plates ignited more than five feet of wire. —which increafed proportionally to the number.

Thefe experiments prove that the powers increafe in a greater ratio than as the furfaces of the plates employed. For four hundred plates of four inches, contain an equal furface with one hundred plates of eight inches; and the former will only ignite two inches of the fame wire, of which the latter will ignite thirty-two inches. Examination of the law of increafe in the power of larger plates.

If this proportion fhould be obferved in experiments with plates of different fize, it will appear that the powers of igniting, as meafured by the length of wire, increafe, in batteries of the fame total furface, as the fquares of the furfaces of the elementary plates, fingly taken in each. By equal total furfaces the wire burned is as the fquare of the elementary furface.

A plate

Reasoning on the facts. Large plates throw our electricity more rapidly.

A plate of eight inches diameter exposes a surface four times greater than a plate of four inches, and supposing the quantities of electricity given out, to be in proportion to the surface exposed to the chemical action of the fluids, the intensity of (or rapidity of evolution from) each plate of an eight inch trough may be estimated as four times greater than the intensity of electricity from the four inch plates; but the power of ignition is sixteen times greater.

Upon these remarkable circumstances I shall venture to submit to you a few conjectures.

The electricity is thrown out by the parts under the process of oxidation,

A simple galvanic combination resembles in its properties a Leyden phial, that side of the metallic plate which undergoes the greatest change resembles the positive side of the jar, and the other side, the negative, there being no other difference between electricity and galvanism, than the mode in which they are produced. Electricity which appears from the excitation of glass, is occasioned by a temporary change in the capacity of the glass for electricity, while under the rubbing action, being momentarily increased; while galvanism is in some respects the reverse, being occasioned by the diminished capacity of good conducting bodies, when exposed to certain chemical changes. Thus metals which are excellent conductors, when oxydated become non-conductors. While they undergo this change a portion of the combined electricity is evolved, and consequently in the part thus changing positive signs will be evinced.

—and thus by electrophoric action produces a negative state on the unoxided surface.

The electricity existing in an unchanged metal being perfectly quiescent, is subject, when disturbed, to those laws to which all material substances are amenable; namely of moving in the direction where it meets with the least resistance. As the direction of the electricity is from the substance of the metal towards the surface undergoing the chemical change, it must follow that the other side of the metallic plate which is defended from the action of the fluid will evince negative signs, while the side acted upon positive, nearly upon the same principles as those of the electraphams.

The discharge is by a succession of correspondent pairs of plates,

In effecting a discharge of the galvanic battery, the circuit must be completed between the zinc and copper ends of the battery. The two plates nearest the points of contact are first concluded; thus the two next, and so on in successive parts, each plate at equal distances from the points of contact; and

4 the

the laſt pair which is unloaded, are the two plates in the centre of the trough. In an arrangement of only 50 or 60 plates, the rapidity of each ſucceſſive diſcharge appears to be ſo great as not to admit of the intervals being diſtinguiſhed by our ſenſe of feeling; but when the number is very conſiderable the ſenſation produced is jarring and tremulous, ſuch as we may conceive would ariſe from the action of a conſtant current of a ſubtile fluid.

—conſequently the whole diſcharge of equal ſurfaces is more rapidly made the fewer the plates,

When we expoſe metallic ſubſtances to the galvanic action, the effects produced are greater from a large ſurface diffuſed over a ſmall number of plates, than when ſpread over a greater number of plates; if we ſuppoſe 800 particles of electricity given out from a plate of eight inches in diameter, there would only be 200 particles from a plate of four inches (ſuppoſing the quantity in the ratio of the ſurfaces expoſed) if two plates be unloaded at once, we ſhall have from one arrangement 1600 particles of electricity determined in one maſs, and from the other only 400; from the former we have a rapid communication of a quantity of electricity of four times the intenſity of the latter. And as metallic ſubſtances nearly tranſmit the whole, the effects produced are found to be as the ſquares of the intenſities.

—and the quantity of electricity actually paſſing in the wire at any inſtant of time is greater.

I am now conſtructing a battery of fifty plates, each plate of which will be two feet in diameter. If the above circle ſhould hold good, the extent of wire that will be ignited will be very conſiderable. A battery of 50 plates, each plate eight inches in diameter, ignites 16 inches of ſteel wire of one ſeventieth of an inch in diameter. And as the plates I am now preparing will expoſe a ſurface nine times greater, I ſhall expect that $16 \times 9 \times 9 = 1296$ inches, or 108 feet of wire will be ignited at every contact.

A battery now in hand expected to explode above 100 feet of wire.

## XI.

*Account of the Changes that have happened, during the last Twenty-five Years, in the relative Situation of Double-stars; with an Investigation of the Cause to which they are owing. By* WILLIAM HERSCHEL, *LL. D. F. R S. From the Philosophical Transactions for* 1803.

*Read June* 9, 1803.

General observations on the theory of the heavenly bodies.

IN the Remarks on the Construction of the Heavens, contained in my last Paper on this subject,* I have divided the various objects which astronomy has hitherto brought to our view, into twelve classes. The first comprehends insulated stars.

As the solar system presents us with all the particulars that may be known, respecting the arrangement of the various subordinate celestial bodies that are under the influence of stars which I have called insulated, such as planets and satellites, asteroids and comets, I shall here say but little on that subject. It will, however, not be amiss to remark, that the late addition of two new celestial bodies, has undoubtedly enlarged our knowledge of the construction of the system of insulated stars. Whatever may be the nature of these two new bodies, we know that they move in regular elliptical orbits round the sun. It is not in the least material whether we call them asteroids, as I have proposed; or planetoids, as an eminent astronomer, in a letter to me, suggested; or whether we admit them at once into the class of our old seven large planets. In the latter case, however, we must recollect, that if we would speak with precision, they should be called very small, and exzodical; for, the great inclination of the orbit of one of them to the ecliptic, amounting to 35 degrees, is certainly remarkable. That of the other is also considerable; its latitude, the last time I saw it, being more than 15 degrees north. These circumstances, added to their smallness, show that there exists a greater variety of arrangement and size among the bodies which our sun holds in subordination, than we had formerly been acquainted with, and extend our knowledge of the construction of the solar, or insulated sidereal system. It will not be required that I should add any

* See Phil. Transf. for 1802, p. 477, and our Journal, V. 72,

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thing farther on the fubject of this firft article of my claffification; I may therefore immediately go to the fecond, which treats of binary fidereal fyftems, or real double ftars. General obfervations on the theory of the heavenly bodies.

We have already fhewn the poffibility that two ftars, whatfoever be their relative magnitudes, may revolve, either in circles or ellipfes, round their common centre of gravity; and that, among the multitude of the ftars of the heavens, there fhould be many fufficiently near each other to occafion this mutual revolution, muft alfo appear highly probable. But neither of thefe confiderations can be admitted in proof of the actual exiftence of fuch binary combinations. I fhall therefore now proceed to give an account of a feries of obfervations on double ftars, comprehending a period of about 25 years, which, if I am not miftaken, will go to prove, that many of them are not merely double in appearance, but muft be allowed to be real binary combinations of two ftars, intimately held together by the bond of mutual attraction.

It will be neceffary to enter into a certain theory, by which thefe obfervations ought to be examined, that we may find to what caufe we fhould attribute fuch changes in the pofition, or diftance, of double ftars, as will be reported; and, in order to make the required principles very clear, I fhall give them in a few fhort and numbered fentences, that they may be referred to hereafter.

In *Plate*. X. *Fig* 1. let us call the place of the fun, which may alfo be taken for that of the obferver, O. In the centre of an orbit or plane N F S P is $\alpha$ Geminorum; and, if any other ftar is to be examined, we have only to exchange the letter $\alpha$ for that by which fuch double ftar is known. This letter is always underftood to reprefent the largeft of the two ftars which make up the double ftar; and a general expreffion for its fmaller companion will be $x$. N, F, S, P, reprefent the pofitions of the different parts of the heavens, with refpect to $\alpha$, north, following, fouth, and preceding; and the fmall letters *n*, *f*, *s*, *p*, ftand for the fame directions with refpect to O. $x\,\alpha$ P, is the angle of pofition of the two ftars $x$ and $\alpha$, with the parallel F P. Theory of the motions of the ftars.

As the motion of an obferver affects the relative fituation of objects, we have three bodies to confider, in our inveftigation of the caufe of the changes which will be pointed out; the fun, the large ftar, and the fmall ftar, or, as we have fhortly called

 them,

Theory of the motions of the ftars.

them, O, $\alpha$, $x$. This admits of three cafes: a motion of one of the three bodies; another, of two; and a third, of all the three bodies together. We fhall now point out the confequences that will arife in each of the cafes.

*Single Motions.*

No. 1. Motion of $x$. When $\alpha$ and O are at reft, the motion of $x$ may be affumed, fo as perfectly to explain any change of the diftance of the two ftars, and of their angle of pofition.

No. 2. Motion of $\alpha$. When $x$ and O are at reft, and $\alpha$ has a motion, either towards P, N, F, or S, then the effect of it, whatever may be the angle P $\alpha$ O, will be had by entering the following Table, with the direction of the given motion.

| Motion. | Diftance. | Angle. | Quadrants. |
|---|---|---|---|
| $\alpha$P | — | + | 1ft and 4th |
| | + | — | 2 —— 3 |
| $\alpha$F | + | — | 1 —— 4 |
| | — | + | 2 —— 3 |
| $\alpha$N | — | — | 1 —— 2 |
| | + | + | 3 —— 4 |
| $\alpha$S | + | + | 1 —— 2 |
| | — | — | 3 —— 4 |

No. 3. Motion of O. 1ft cafe. When $\alpha$ and $x$ are at reft, and the angle P $\alpha$ O is 90 degrees, a proper motion of O, towards either $p$, $f$, $n$, or $s$, which will be extremely fmall when compared with the diftance of O from $\alpha$, can have no effect on the apparent diftance, or angle of pofition, of the two ftars; and therefore no other motion, compofed of the directions we have mentioned, will induce a change in the comparative fituation of $\alpha$ and $x$.

2d cafe. When the plane P N F S is oblique to the ray $\alpha$ O, and the angle P $\alpha$ O more than 90 degrees, the effect of the motion of O will be had by the following Table.

Motion

Theory of the motions of the ftars.

| Motion. | Diftance. | Angle. | Quadrants. |
| --- | --- | --- | --- |
| O $p$ | + | — | 1ft and 2d<br>3 —— 4 |
| O $f$ | — | + | 1 —— 2<br>3 —— 4 |
| O $n$ | +<br>— | +<br>— | 1 —— 3<br>2 —— 4 |
| O $s$ | —<br>+ | —<br>+ | 1 —— 3<br>2 —— 4 |

3d cafe. When the angle P $\alpha$ O is lefs than 90 degrees, the following Table muft be ufed.

| Motion. | Diftance. | Angle. | Quadrants. |
| --- | --- | --- | --- |
| O $p$ | — | + | 1ft and 2d<br>3 —— 4 |
| O $f$ | + | — | 1 —— 2<br>3 —— 4 |
| O $n$ | —<br>+ | —<br>+ | 1 —— 3<br>2 —— 4 |
| O $s$ | +<br>— | +<br>— | 1 —— 3<br>2 —— 4 |

*Double Motions.*

No. 4. If we admit different motions in two of our three bodies, and if the ratio of the velocities, the directions of the motions, and the ratio of the diftances of the bodies be given quantities, a fuppofition in which we admit their concurrence, may explain the phenomena of a double ftar, but can never be probable

*Motions of the three Bodies.*

No. 5. If we admit different motions in every one of the three bodies, O $\alpha$, $x$, and if the velocities and directions of the motions,

Theory of the motions of the stars.

motions, as well as the relative distances of the three bodies are determined, an hypothesis which admits the existence of such motions and situations, may resolve the phenomena of a double star, but cannot have any pretension to probability.

The compass of this Paper will not allow me to give the observations of my double stars at full length; I shall therefore, in the examination of every one of them, only state those particulars which will be required for the purpose of investigating the cause of the changes that have taken place, either in the distance, or angle of position, of the two stars of which the double star is composed.

As the arguments in the case of most of these stars will be nearly the same, it may be expected, that the first two or three which are to be examined will take up a considerable space, and the number of double stars, in which I have already ascertained a change, amounting to more than fifty, it will not be possible to give them all in one paper; I shall therefore confine the present one to a moderate length, and leave it open for a continuation at a future opportunity.

*α Geminorum.*

Observations and inferences respecting the changes of relative situation in stars extremely near each other.

From my earliest observations on the distance of the two stars which make up the double star in the head of Castor, given in the first of my catalogues of double stars, we find, that about 23 years and a half ago, they were nearly two diameters of the large star asunder. These observations have been regularly continued, from the year 1778 to the present time, and no alteration in the distance has been perceived: the stars are now still nearly two diameters of the large one asunder.

It will be necessary to enter a little into the practicability of ascertaining distances by a method of estimation apparently so little capable of precision. From a number of observations and experiments I have made on the subject, it is certain that the apparent diameter of a star, in a reflecting telescope, depends chiefly upon the four following circumstances: the aperture of the mirror with respect to its focal length; the distinctness of the mirror; the magnifying power; and the state of the atmosphere at the time of observation. By a contraction of the aperture we can increase the apparent diameter of a star, so as to make it resemble a small planetary disk. If distinctness should be wanting, it is evident that the image of objects will not be sharp

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and well defined, and that they will consequently appear larger than they ought. The effect of magnifying power is, to occasion a relative increase of the vacancy between two stars that are very near each other; but the ratio of the increase of the distance is not proportional to that of the power, and sooner or later comes to a maximum. The state of the atmosphere is perhaps the most material of the four conditions, as we have it not in our power to alter it. The effects of moisture, damp air, and haziness, (which have been related in a paper where the causes that often prevent the proper action of mirrors were discussed,) show the reason why the apparent distance of a double star should be affected by a change in the atmosphere. The alteration in the diameter of Arcturus, extending from the first to the last of the ten images of that star, in the plate accompanying the above-mentioned paper,* shows a sufficient cause for an increase of the distance of two stars, by a contraction of their apparent disks. A skilful observer, however, will soon know what state of the air is most proper for estimations of this kind. I have occasionally seen the two stars of Castor, from $1\frac{1}{2}$ to 2 and $2\frac{1}{2}$ diameters asunder; but, in a regular settled temperature and clear air, their distance was always the same. The other three causes which affect these estimations, are at our own disposal; an instance of this will be seen in the following trial. I took ten different mirrors of seven feet focal length, each having an aperture of 6,3 inches, and being charged with an eye-glass which gave the telescope a magnifying power of 460. With these mirrors, one after another, the same evening I viewed the two stars of our double star; and the result was, that with every one of them, the stars were precisely at an equal distance from each other. These mirrors were all sufficiently good to show minute double stars well; and such a trial will consequently furnish us with a proper criterion, by which we may ascertain the goodness of our telescope, and the clearness of the atmosphere required for these observations. To those who have not been long in the habit of observing double stars, it will be necessary to mention, that, when first seen, they will appear nearer together than after a certain time; nor is it so soon as might be expected, that we see them at their greatest

* See Phil. Transf. for 1803, page 232, Plate III.

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diſtance. I have known it to take up two or three months, before the eye was ſufficiently acquainted with the object, to judge with the requiſite preciſion.

Whatever may be the difficulties or uncertainties, attending the method of determining the diſtance of two cloſe ſtars by an eſtimation of the apparent diameter, it muſt however be confeſſed, that we have no other way of obtaining the ſame end with ſo much preciſion. Our preſent inſtance of $\alpha$ Geminorum, will ſhow the degree of accuracy of which ſuch eſtimations are capable, and at the ſame time prove, that the purpoſe for which I ſhall uſe the eſtimated interval between the two ſtars will be ſufficiently anſwered. By an obſervation of the 10th of May, 1781, we have the diameter of the largeſt of the two ſtars to that of the ſmalleſt as 6 to 5; and according to ſeveral meaſures I have taken with the micrometer, we may admit their diſtance, diameters included, to be five ſeconds. Then, as the vacancy between the two ſtars is nearly, but not quite two diameters of the large one, I ſhall value it at $1\frac{7}{8}$. From this we calculate, that the diameter of the large ſtar, under the circumſtances of our eſtimation, is nearly $1'',35$: ſo that an error of one quarter of ſuch a diameter, which is the moſt we can admit, will not exceed $0'',34$. Nor is it of much conſequence, if the meaſure of $5''$ ſhould not be extremely correct; as a ſmall miſtake in that quantity will not materially affect the error of eſtimation by the diameter, which, from what has been ſaid, if the meaſure was faulty to a ſecond, would not amount to more than one-fifteenth part of it.

Having thus aſcertained that no perceptible change in the diſtance of the ſtars has taken place, we are now to examine the angle of poſition. In the year 1779, it was 32° 47′ north preceding; and, by a mean of the three laſt meaſures I have taken, it is now only 10° 53′. In the ſpace of about 23 years and a half, therefore, the angle of poſition has manifeſtly undergone a diminution, of no leſs than 21° 54′; and, that this change has been brought on by a regular and gradual decreaſe of the angle, will be ſeen when the reſt of the meaſures come to be examined.

The accuracy of the micrometer which has been uſed, when the angles of poſition were taken, being of the utmoſt importance, it becomes neceſſary to aſcertain how far it will be ſafe to rely on the reſult of the meaſures. It might be eaſily

ſhown

ſhown that, in the day time, a given angle, delineated on a card, and ſtuck up at a convenient diſtance, may be full as accurately meaſured by a teleſcope furniſhed with this micrometer, as it can be done by any known method, when the card is laid on a table before us; but this would not anſwer my purpoſe. For, objects in motion, like the ſtars, eſpecially when at a diſtance from the pole, cannot be meaſured with ſuch ſteadineſs as thoſe which are near us, and at reſt. The method of illuminating the wires, and other circumſtances, will likewiſe affect the accuracy of the angles that are meaſured, eſpecially when the diſtance of the ſtars is very ſmall. I ſhall therefore have recourſe to aſtronomical obſervations, in order to ſee what the micrometer has actually done.

Obſervations and inferences reſpecting the changes of relative ſituation in ſtars extremely near each other.

January 22, 1802. The poſition of A Orionis was taken. 1ſt meaſure, 52° 38′ ſouth preceding; 2d meaſure, 54° 14′. Mean of the two meaſures, 53° 26′. Deviation of the meaſures from the mean, 48′.

March 4, 1802. 11 Monocerotis. 1ſt meaſure, 28° 18′ ſouth following; 2d meaſure, 26° 49′. Mean of the two, 27° 34′. Deviation from the mean, 45′.

February 9, 1803. α Geminorum. 1ſt meaſure, 6° 11′ north preceding; 2d meaſure, 4° 48′. Mean of the two, 5° 29′. Deviation from the mean, 41′.

September 6, 1802. η Coronæ. 1ſt meaſure, 89° 42′ north following; 2d meaſure, 89° 38′. Mean of the two, 89° 40′. Deviation from the mean, 2′.

When theſe obſervations are conſidered, we ſhall not err much if we admit that, in favourable circumſtances, and with proper care, the micrometer, by a mean of two meaſures, will give the poſition of a double ſtar true to nearly one degree; but, as the opportunities of taking very accurate meaſures are ſcarce, it will be neceſſary to have recourſe to ſome more diſcordant obſervations.

February 18, 1803. β Orionis. 1ſt meaſure, 72° 58′ ſouth preceding; 2d meaſure, 67° 24′. Mean of the two, 70° 11′. Deviation from the mean, 2° 47′.

But a memorandum to the obſervation ſays, that the evening was not favourable. We may therefore admit, that in the worſt circumſtances which can be judged proper for meaſuring at all, an error in the angle of poſition by two meaſures, will not amount to three degrees.

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It will be remarked, when we come to compare fingle meafures which have been taken on different nights, that they are fomewhat more difcordant; but I have not ventured to reject them on that account, except in cafes where it was pretty evident that fome miftake in reading off, or other accident to which all aftronomical obfervations are liable, was to be apprehended. Nor can fuch difagreements materially affect the conclufions I have drawn, when it appears that the deviations happen fometimes to be on one fide, and fometimes on the other fide, of the true angle of pofition. For, fince that angle is not a thing that will change in the courfe of a few nights, the excefs of one meafure will ferve to correct the defect of another; and we are not to think it extraordinary, when ftars are fo near together, and their motion through the field of view (in confequence of the high magnifying power we are obliged to ufe) fo quick, that we fhould now and then even fall fhort of that general accuracy which may be had by a careful ufe of the micrometer.

I fhall now enter into an examination of the caufe of the change in the angle of pofition of the fmall ftar near Caftor.

A revolving ftar, it is evident, would explain in a moft fatisfactory manner, a continual change in the angle of pofition, without an alteration of the diftance. But this, being a circumftance of which we have no precedent, ought not to be admitted without the fulleft evidence. It will therefore be right to examine, whether the related phenomena cannot be fatisfactorily explained by the proper motions of the ftars, or of the fun.

*Single Motions.*

($a$) The three bodies we have to confider, are O, $\alpha$, and $x$; and, fuppofing them to be placed as they were obferved to be in the year 1779; the angle $x\,\alpha$ P, in Fig. 1, will be 32° 47′ north preceding. We are at liberty to let the angle P $\alpha$ O be what will beft anfwer the purpofe. Then, in order to examine the various hypothefes that may be formed, according to the arrangement of the principles we have given, we fhall begin with No. 1; and, as this admits that all phenomena may be refolved by a proper motion of $x$, let us fuppofe this ftar to be placed any where far beyond $\alpha$, but fo as to have been feen, in the year 1779, where the angle of pofition, 32° 47′ north preceding

ceding, and the obferved diftance, near 2 diameters of the large ftar, required it. With a proper velocity, let it be in motion towards the place where it may now be feen at the fame diftance from Caftor, but under an angle of pofition only 10° 53′ north preceding. It may then be admitted, that a fmall decreafe of the diftance which would happen at the time when the angle of pofition was 21° 50′, could not have been perceived; fo that the gradual change in the obferved angle of pofition, as well as the equality of the diftance of the two ftars, will be fufficiently accounted for. But the admiffion of this hypothefis requires, that α Geminorum and the folar fyftem fhould be at reft; and, by the obfervations of aftronomers, which I fhall foon have occafion to mention, neither of thefe conditions can be conceded.

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(*b*) If, according to No. 2, we admit the motion of α, we fhall certainly be more confiftent with the obfervations which aftronomers have made on the proper motion of this ftar *; and, as a motion of the folar fyftem, which I fhall have occafion to mention hereafter, has not been rigidly proved, it may, for the fake of argument, be fet afide; nor has a proper motion of the ftar *x* been any where afcertained. The retrograde annual proper motion of Caftor, in right afcenfion, according to Dr. Mafkelyne, is 0″,105. This, in about 23½ years, during which time I have taken notice of the angle of pofition and diftance of the fmall ftar, will amount to a change of nearly 2″,47. Then, if we enter the fhort Table I have given in No. 2, with the motion αP, we find, that in the firft quadrant, where the fmall ftar is placed, the diftance between the two ftars will be diminifhed, and the angle of pofition increafed. But fince it appears, by my obfervations, that the diftance of the ftars is not lefs now than it was in 1780; and that, inftead of an increafe in the angle of pofition, it has actually undergone a diminution of nearly 22 degrees; it follows, that the motion of α Geminorum in right afcenfion, will not explain the obferved alterations in the fituation of this double ftar. If,

* See Tobiæ Mayeri *Opera inedita. De motu fixarum proprio*, page 80. Alfo Dr. Maskelyne's firft Volume of Obfervations. Explanation and Ufe of the Tables, p. iv. Or Mr. Wollafton's Aftronomical Catalogue, end of the Preface. Likewife *Connoiffance des Temps pour l'Annèe* VI. p. 203. *Sur le Mouvement particulier propre a differentes Etoiles*; par Mons. De la Lande.

according

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according to Mr. De la Lande's account*, we ſhould alſo conſider the annual proper motion of $\alpha$ in declination, which is given 0″,12 towards the north, we ſhall find, by entering our Table with the motion $\alpha$ N, amounting to 2″82, that the diſtance of the two ſtars will be ſtill more diminiſhed; but that, on the contrary, the angle of poſition will be much leſſened; and, by combining the two motions together, the apparent diſks of the two ſtars ſhould now be a little more than one-tenth of a ſecond from each other, and the angle of poſition 35 degrees ſouth preceding. But, ſince neither of theſe effects have taken place, the hypotheſis cannot be admitted.

*(c)* That the ſun has a proper motion in ſpace, I have ſhown with a very high degree of evidence, in a paper which was read at the Royal Society about twenty years ago †. The ſame opinion was before, but only from theoretical principles, hinted at by Mr. De la Lande, and alſo by the late Dr. Wilſon, of Glaſgow ‡; and has, ſince the publication of my paper, been taken up by ſeveral aſtronomers§, who agree that ſuch motion exiſts. In conſequence of this, let us now, according to No. 3. aſſign to the ſun a motion in ſpace, of a certain velocity and direction. Admitting therefore $\alpha$ and $x$ to be at reſt, let the angle P$\alpha$O be 90 degrees; then, by the 1ſt caſe of No. 3, we find that none of the obſerved changes of the angles of poſition will admit of an explanation. There is moreover an evident conceſſion of the point in queſtion, in the very ſuppoſition of the above angle of 90 degrees; for, if $x$ be at the ſame diſtance as $\alpha$ from the ſun, and no more than 5″ from that ſtar, its real diſtance, compared to that of the ſun from the ſtar, will be known; and, ſince that muſt be leſs than the 40 thouſandth part of our diſtance from Caſtor, theſe two ſtars muſt neceſſarily be within the reach of each others attraction, and form a binary ſyſtem.

*(d)* Let us now take the advantage held out by the 2d caſe of No. 3, which allows us to place $x$ far behind $\alpha$; in which

* See page 211 of the treatiſe before referred to.

† Philoſ. Tranſ. vol. lxxiii. p. 247.

‡ See my note in Phil. Tranſ. vol. lxxiii. p. 283.

§ See *Aſtronomiſches Jahrbuch für das Jahr* 1786; *ſeite* 259. *Uber die fortruckung unſeres Sonnen-Syſtems, von* Herrn *Profeor* Prevoſt. *Und für das Jahr* 1805; *ſeite* 113.

ſituation

situation, the angle P$\alpha$O will be more than 90 degrees. The star $x$ being less than $\alpha$, renders this hypothesis the more plausible. Now, as a motion of Castor, be it real or apparent, has actually been ascertained, we cannot set it aside; the real motion of O, therefore, in order to account for the apparent one of $\alpha$, must be of equal velocity, and in a contrary direction: that is, when decomposed, 0″,105 towards $f$, and 0″,12, towards $s$. The effect of the sun's moving from O towards $f$, according to the 1st Table in No. 3, is, that the distance between the two stars will be diminished, and the angle of position increased. But these are both contrary to the observations I have given. The motion of O in declination towards $s$, according to the same Table, will still diminish the distance of the two stars, but will also diminish the angle of position. Then, since a motion in right ascension increases the angle, while that in declination diminishes it, the small star may be placed at such a distance that the difference in the parallax, arising from the solar motion, shall bring the angle of position, in 23½ years, from 32° 47′ to 10° 53′; which will explain the observed change of that angle. The distance of the star $x$, for this purpose, must be above 2½ times as much as that of $\alpha$ from us. But, after having in this manner accounted for the alteration of the angle of position, we are, in the next place, to examine the effect which such a difference of parallax must produce in the apparent distance of the two stars from each other. By a graphical method, which is quite sufficient for our purpose, it appears, that the union of the two motions in right ascension and declination, must have brought the two stars so near, as to be only about half a diameter of the large star from each other; or, to express the same in measures, the centers of the stars must now be 1″,8 nearer than they were 23½ years ago. But this my observations cannot allow; for we have already shown, that any change of more than 3 or 4-tenths of a second must have been perceived.

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If, on the other hand, we place the star $x$ at such a distance that the solar parallax may only bring it about 4-tenths of a second nearer to $\alpha$, which is a quantity we may suppose to have escaped our notice in estimating the apparent distance of the two stars, then will the angle of position be above 20 degrees too large. This shows, that no distance, beyond Castor, at which we can place the star, will explain the given observations.

(c)

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(e) The laft remaining trial we have to examine, is to fuppofe $x$ to be nearer than $\alpha$; the angle P $\alpha$ O, will then be lefs than 90 degrees; and the effect of a motion of O towards $f$, by the 2d Table in No. 3, will be an increafe of the diftance of the two ftars, and a diminution of their angle of pofition. But the motion O$s$, which is alfo to be confidered, will add to the increafe of the diftance and counteract the diminution of the angle. It is therefore to be examined, whether fuch an increafe of diftance as we can allow to have efcaped obfervation, will explain the change which we know to have happened in the angle, during the laft 23½ year. By the fame method of compounding the two motions as before, it immediately appears, than we cannot place the fmall ftar more than about 1-tenth of the diftance O$\alpha$ on the fide of Caftor, without occafioning fuch an increafe of the apparent diftance of the two ftars as cannot poffibly be admitted; and that, even then, the angle of pofition, inftead of being lefs, will be a few degrees larger, at the end of 23½ years, than it was at the beginning. This hypothefis, therefore, like all the foregoing ones, muft alfo be given up, as inconfiftent with my obfervations.

It is moreover evident, that the obfervations of aftronomers on the proper motion of the ftars in general, will not permit us to affume the folar motion at pleafure, merely for the fake of accounting for the changes which have happened in the appearances of a double ftar. The proper motion of Caftor, therefore, cannot be intirely afcribed to a contrary motion of the fun. For we can affign no reafon why the proper motion of this ftar alone, in preference, for inftance, to that of Arcturus, of Sirius, and of many others, fhould be fuppofed to arife from a motion of the folar fyftem. Now, if they are all equally intitled to partake of this motion, we can only admit it in fuch a direction, and of fuch a velocity, as will fatisfy mean direction and velocity of the general proper motions of the ftars; and place all deviations to the account of a real proper motion in each ftar feparately.

*Double Motion.*

($f$) In order to explain the phenomena of our double ftar, according to No. 4, by the motion of two bodies, for inftance $\alpha$ and $x$, it will be required that they both fhould move in given

directions; that the velocities of their motions ſhould be in a given ratio to each other; and that this ratio ſhould be compounded with the ratio of their diſtances from O; a ſuppoſition which muſt certainly be highly improbable. To ſhew this with ſufficient evidence, let us admit that, according to the beſt authorities, the annual proper motion of Caſtor is —0″,105 in right aſcenſion, and 0″,12 in declination towards the north. Then, as the ſmall ſtar, without changing its diſtance, has moved through an angle of 21° 54′, the only difference in the two motions of theſe ſtars, will be expreſſed by the extent of the chord of that angle. To produce the required effect, it is therefore neceſſary that the motion of $\alpha$, which is given, ſhould regulate that of the ſmall ſtar, whoſe relative place at the end of 23½ years is alſo given. Then, as $\alpha$ moves in angle of 53° 31′ north preceding, and with a velocity which, being expreſſed by the ſpace it would deſcribe in 23½ years, will be 3″,51, it is required that $x$ ſhall move in an angle of 29° 25′, likewiſe north preceding, and with a velocity of 3″,02. The ratio of the velocities, therefore, and the directions of the motions, are equally given. But this will not be ſufficient for the purpoſe: their diſtance from O muſt alſo be taken into conſideration. It has been ſhown, that the two ſtars cannot be at an equal diſtance from us, without an evident connection; it will therefore be neceſſary for thoſe who will not allow this connection, to place one of them nearer to us than the other. But, as the motions which have been aſſumed, when ſeen from different diſtances, will ſubtend lines whoſe apparent magnitudes will be in the inverſe ratio of the aſſumed diſtances, it is evident that this ratio, if the motions are given, muſt alſo be a given one; or that, if the diſtances be aſſumed, the ratio of the motions muſt be compounded with the ratio of the diſtances. How then can it be expected that ſuch preciſe conditions ſhould be made good, by a concurrence of circumſtances owing to mere chance? Indeed, if we were inclined to paſs by the difficulties we have conſidered, there is ſtill a point left which cannot be ſet aſide. The motion of the ſolar ſyſtem, although its preciſe direction and velocity may ſtill be unknown, can hardly admit of a doubt; we have therefore a third motion to add to the former two, which conſequently will bring the caſe under the ſtatement contained in our 7th number, and will be conſidered hereafter.

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(*g*) If we fhould intend to change our ground, and place the two motions in O and $x$, it will then be conceded, that the motion of $\alpha$ is only an apparent one, which owes its exiftence to the real motion of the fun. By this, the effect of the folar parallax on any ftar at the fame diftance will be given; and it cannot be difficult to affume a motion in $x$, which fhall, with the effect of this given parallax, produce the apparent motion, in the direction of a chord from the firft to the laft angle of pofition pointed out by my obfervations; taking care, however, not to place the ftars $\alpha$ and $x$ at the fame diftance from us; ufing the inverfe ratio of the folar parallax as a multiple in the affigned motion. For inftance, let the fun have a motion of the velocity expreffed as before by 3″,51, and in a direction which makes an angle of 53°, 13′ fouth following with the parallel of $\alpha$ Geminorum; and let the fmall ftar $x$ have a real motion in an angle of 18° 40′ fouth preceding from the parallel of its fituation, and with a real velocity which, were it at the diftance of $\alpha$, would carry it through 8″,89. Then, if the diftance of the fmall ftar be to that of the large one as 3 to 2, the effect of the folar parallax upon it will be $\frac{2}{3}$ of its effect upon $\alpha$; that is, while $\alpha$, which is at reft, appears to move over a fpace of 3″,51, in an angle of 53° 31′ north preceding, the parallactic change of place in $x$ will be 2″,34 in the fame direction. This, though only an apparent motion, will be compounded with the real motion we have affigned to it, but which, at the diftance of $\alpha$, will only appear as 1″,26; and the joint effect of both will bring the ftar from the place in which it was feen $23\frac{1}{2}$ years ago, to that where now we find it fituated. $\alpha$, in the fame time, will appear to have had an annual proper motion of — 0″,105 in right afcenfion, and 0″,12 in declination towards the north; and thus all phenomena will be explained.

From this ftatement, we may draw a confequence of confiderable importance. If we fucceed, in this manner, in accounting for the changes obferved in the relative fituation of the two ftars of a double ftar, we fhall fail in proving them to form a binary fyftem; but, in lieu of it, we fhall gain two other points, of equal value to aftronomers. For, as $\alpha$ Geminorum, according to the foregoing hypothefis, is a ftar that has no real motion, its apparent motion will give us the velocity and direction of the motion of the folar fyftem; and, this being

being obtained, we ſhall alſo have the relative parallax of every ſtar, not having a proper motion, which is affected by the ſolar motion. Aſtronomical obſervations on the proper motion of many different ſtars, however, will not allow us to account for the motion of $\alpha$ Geminorum in the manner which the foregoing inſtance requires; the hypotheſis, therefore, of its being at reſt, muſt be rejected.

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($h$) If we place our two motions in O and $\alpha$, we ſhall be led to the ſame concluſion as in the laſt hypotheſis. The known proper motion of $\alpha$, and the ſituations of the ſmall ſtar in 1779 and 1803, given by my obſervations, will aſcertain the apparent motion of $x$, now ſuppoſed to be at reſt. Then, ſince the change in the place of $x$ muſt be intirely owing to the effect of parallax, it will conſequently give us, in the ſame manner as before, the quantity and direction of the motion of the ſolar ſyſtem, and the relative diſtances of all ſuch ſtars as are affected by it. But, here again, the ſolar motion required for the purpoſe is ſuch as cannot be admitted; and the hypotheſis is not maintainable.

*Motion of the three Bodies.*

($i$) There is now but one caſe more to conſider, which is, according to No. 5, to aſſign real motions to all our three bodies; and this may be done as follows. Suppoſe the ſun to move towards $\lambda$ Herculis, with the annual velocity 1.

Let the apparent motion of $\alpha$ Geminorum be as it is ſtated in the aſtronomical tables before mentioned; but ſuppoſe it to ariſe from a compoſition of its real motion with the effect of the ſyſtematical parallax, as we may call that apparent change of place of ſtars which is owing to the motion of the ſolar ſyſtem. Let the real motion of $x$, aided by the effect of the ſame parallax, be the cauſe of the changes in the angle of poſition which my obſervations have given. We may admit the largeſt of the two ſtars of our double ſtar to be of the ſecond magnitude; and, as we are not to place $x$ too near $\alpha$, we may ſuppoſe its diſtance from O to be to that of $\alpha$ from the ſame as 3 to 2. In this caſe, O will move from the parallel of $\alpha$, in an angle of 60° 37′ north following, with an apparent annual velocity of ,4536. The motion of $\alpha$ in right aſcenſion, may be intirely aſcribed to ſolar parallax; but its change of declination, cannot be accounted for in the ſame manner. Let us therefore


admit

Obfervations and inferences refpecting the changes of relative fituations in ftars extremely near each other.

admit that the folar velocity, in the direction we have calculated, will produce an apparent retrogate motion in $\alpha$, which, in $23\frac{1}{2}$ years will amount to 2″,085 in right afcenfion. But the fame parallax will alfo occafion a change in declination, towards the fouth preceding, of 3″,701; and, as this will not agree with the obferved motion of $\alpha$, we muft account for it by a proper motion of this ftar directly towards the north. The real annual velocity required for this purpofe, muft be 1,3925.

The apparent motion of $x$, by parallax, at the diftance we have placed this ftar, will be 2″,832 towards the fouth preceding; and, by affigning to it an annual proper motion of the velocity 1,3354, in the direction of 73° 10′ north preceding its own parallel, the effect of the folar parallax and this proper motion together, will have caufed the fmall ftar, in appearance, to revolve round $\alpha$, fo as to have produced all the changes in the angle of pofition which my obfervations have given; and, at the fame time, $\alpha$ will have been feen to move from its former place, at the annual rate of 0″,105 in right afcenfion, and 0″,12 in declination towards the north.

In this manner, we may certainly account for the phenomena of the changes which have taken place with the two ftars of $\alpha$ Geminorum. But the complicated requifites of the motions which have been expofed to our view, muft furely compel every one who confiders them to acknowledge, that fuch a combination of circumftances involves the higheft degree of improbability in the accomplifhment of its conditions. On the other hand, when a moft fimple and fatisfactory explanation of the fame phenomena may be had by the effects of mutual attraction, which will fupport the moving bodies in a permanent fyftem of revolution round a common centre of gravity, while at the fame time they follow the direction of a proper motion which this centre may have in fpace, it will hardly be poffible to entertain a doubt to which hypothefis we ought to give the preference.

As I have now allowed, and even fhown, the poffibility that the phenomena of the double ftar Caftor may be explained by proper motions, it will appear that, notwithftanding my foregoing arguments in favour of binary fyftems, it was neceffary, on a former occafion, to exprefs myfelf in a conditional man-

ner,* when, after having announced the contents of this Paper, I added, "*should these observations be found sufficiently conclusive;*" for, if there should be astronomers who would rather explain the phenomena of a small star appearing to revolve round Castor by the hypothesis we have last examined, they may certainly claim the right of assenting to what appears to them most probable.

*(To be continued.)*

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## XII.

*On the Nature of the Varieties of Engrafted Fruit-Trees, with a Plan for increasing the Number of New valuable Fruits.* By T. S. Dyot Bucknall, *Esq. M. P*†.

To Mr. CHARLES TAYLOR.

SIR,

SOME friends have requested that I would introduce another Paper on the Nature of the valuable Varieties of engrafted Fruits, as they are of opinion that the Essay in the 17th Volume of the Transactions of the Society is not sufficiently extended for a subject so important to the Fruit-growers, and those interested in the productions of Fruits. As a proof of my willingness to make the Orchardist as perfect as I can, I beg you to present my compliments to the Society, with the following elucidations. **Introduction.**

This is a subject in rural oeconomy which ought to be much better understood than it is, in order to enable the planters to judge of the sorts proper to be planted, either as an article of pleasure, profit, or recreation; as much of the credit of the plantation must arise from judiciously choosing Trees of the best, new, or middled-aged sorts, and not of the old worn-out varieties, which latter cannot, in the planting of Orchards in common situations, ever form *valuable Trees*, and must end in the disappointment of the planter. **Importance.**

Engrafted Fruits, I have before said, and I now repeat, are not permanent. Every one of the least reflection must see that **Grafted fruits are not permanent.**

* See Phil. Trans. for 1802, page 486.

† Society of Arts, xx. 144.



there

there is an effential difference between the power and energy of a feedling plant, and the tree which is to be raifed from cuttings or elongations. The feedling is endued with the energies of nature, while the graft, or fcion, is nothing more than a regular elongation, carried perhaps through the feveral repeatings of the fame variety; whereas the feed, from having been placed in the earth, germinates and becomes a new plant, wherever nature permits like to produce like in vegetation; as in the oak, beech, and other maft-bearing trees. Thefe latter trees, from each paffing through the ftate of feedlings, are perfectly continued, and endued with the functions of forming perfect feeds for raifing other plants by evolution, to the continuance of the like fpecies.

The varieties continued by grafting laft only for a time.

This is not the cafe with engrafted fruits. They are doomed by nature to continue for a time, and then gradually decline, till at laft the variety is totally loft, and foon forgotten, unlefs recorded by tradition, or in old publications.

Reafon, with which Providence has moft bountifully bleffed fome of our fpecies, has enabled us, when we find a fuperior variety, to engraft it on a wilding ftock, or to raife plants from layers and cuttings, or even to raife up the roots, and thus to multiply our fources of comfort and pleafure. This, however, does not imply that the multiplication of the fame variety, for it is no more, fhould laft for ever, unlefs the fpecies will naturally arife from feed.

Procefs of raifing fruits, and increafing them by grafting.

Nature, in her teaching, fpeaks in very intelligible language, which language is conveyed by experience and obfervation. Thus we fee that among promifcuous feeds of fruits of the fame fort, one or more may arife, whofe fruits fhall be found to poffefs a value far fuperior to the reft in many diftinguifhable properties. From experience, alfo, we have obtained the power, by engrafting, of increafing the number of this newly-acquired tree, can change its country, give it to a friend, fend it beyond the feas, or fill a kingdom with that fruit, if the natives are difpofed fo to do. Thus we feem to have a kind of creative power in our own hands.

From the attention lately paid to the culture of engrafted Fruits, I hope we are now enabled to continue a fuppofed happily acquired tree, when we can find it, for a much longer duration than if fuch variety had been left in the ftate of unaffifted nature; perhaps I may fay for a duration as long again, or

or ſomething more. After theſe ſanguine expectations, I may reaſonably be aſked, to what does all this amount? for here there is no direct permanency—and why? The *why* is very obvious—becauſe the kernels within the fruit, which are the ſeed of the plants for forming the next generation of trees, will not produce their like. I allow they may do ſo accidentally; but nothing more can be depended on.

Proceſs of raiſing fruits and increaſing them by grafting.

For example, ſuppoſe we take ten kernels or pips of any apple raiſed on an engrafted ſtock: ſow them, and they will produce ten different varieties, no two of which will be alike; nor will either of them cloſely reſemble the fruit from whence the ſeeds were collected. The leaves alſo of thoſe trees raiſed from the ſame primogenious or parent ſtock, will not *actually* be a copy of the leaves of any one of the varieties or family, to which each is connected by a vegetable conſanguinity. I intentionally uſed the word *actually*, becauſe a reſemblance may be found, though not much of that is to be expected.

I beg that what has been laſt mentioned may not be taken as a diſcouragement to attempts for raiſing new varieties. I was obliged to ſpeak very ſtrongly, in order to place the culture upon its true foundation. I think it need not be obſerved, that there is no acquiring a new variety, but through the means of a ſeedling plant; and therefore whoever wiſhes to ſucceed muſt attempt it that way, or wait till others in their plantations may more fortunately produce it.

In chooſing the ſeeds, that apple is moſt likely to produce the cleareſt and fineſt plants, whoſe kernels are firm, large, and well ripened. The ſize of the fruit is not to be regarded; for large apples do not always ripen their fruit well, or rather for cider the ſmall fruits are generally preferred for making the ſtrongeſt, higheſt-flavoured liquor. And from what I have been able to collect in the cider-countries, it is there the opinion, that an apple ſomething above the improved crab promiſes the beſt ſucceſs. This advantage alſo attends the practice: if there are no valuable apples raiſed from that attempt, theſe wildings will make excellent ſtocks to engraft upon.

Gentlemen who actually employ themſelves in attempting to acquire new varieties, ſhould remember that they ought to ſelect all the ſets, from the bed of apple-quick, whoſe appearance is in the leaſt degree promiſing, and plant them together, at ſuch a diſtance as to allow each to produce its fruit, which will

will happen in about twelve, fifteen, or eighteen years. My friend Mr. Knight, who undoubtedly is the firft in actual exertions for procuring thefe happily acquired new varieties, has had two plants bear fruit at fix years old, and one at five. The cider-countries have offered feveral premiums for procuring new varieties, and fome with good effect. Premiums have been given both to Mr. Knight and Mr. Alban.

When the new variety is to be raifed from a valuable *admired* apple, I fhould recommend the placing thefe feeds in a garden-pot, filled with mould from an old melon-bed; carrying the pot into a retired fituation near the water, and giving attention to run the plants to as large a fize as is convenient within eighteen months. With this view, the pot fhould be placed in the green-houfe the firft winter; and when the plants are afterwards to be fet out in the fpots, they fhould not be placed under the drip of trees, or much expofed to the winds.

Two inftances have been mentioned, the improved crab, and moft admired apple; but prudence fays, try all forts, and fomething probably will arife; and the procefs is attended with little trouble or expence to a perfon who conftantly refides in the country: yet, after all this fcientific care, the apple may want flavour, and be in other refpects nothing better than a common wilding.

It is an undoubted fact, and worthy of obfervation, that all the different trees of the fame variety have a wonderful tendency to fimilarity of appearance among themfelves; and that the parent ftock, and all engrafted from it, have a far greater refemblance to each other, that can be found in any part of the animal creation; and this habit does not vary to any extent of age.

Facts refpecting engrafted fruits, their degree of permanence, &c.

As an encouragement in attempting to increafe the number of new valuable fruits, we can prove that the golden pippin is native Englifh. The red-ftreak, a feedling of Herefordfhire, if not raifed, yet was firft brought into notice by Lord Scudamore, and was for a long time called Scudamore's Crab. The Stire Apple was accidentally raifed in the Foreft of Dean, in Gloucefterfhire, and took the name of *Foreft Stire*. The cider made from this apple was the ftrongeft the country ever produced, according to any living record. The Haglo-crab, the beft cider fruit now remaining, was difcovered

5 in

in the parifh of Ecloe, on the banks of the Severn; and, about fixty or feventy years ago, many fcions were taken from this tree by Mr. Bellamy, and engrafted on feedling ftocks about Rofs. Thefe are now grown old; and, to afcertain the age of the variety, I went with Charles Edwin, Efq. to Ecloes, in hopes of feeing the primogenious of this family. The proprietor of the eftate acquainted Mr. Edwin that it had ceafed to bear years ago, and was cut down. Thofe at Rofs are but poor bearers now, and I fhould fuppofe the variety muft be 140 years old, though Marfhall, who wrote in the year 1786, mentions thefe trees were prolific, and he fuppofes the fort to be about eighty years old; but, from prefent experience, it muft be much more. The Tinton Squafh-pear is of Gloucefterfhire; the Barland and Old-field were near Ledbury, Herefordfhire. The two laft pears clearly bear the names of the two fields where they were raifed. The Barland fell about fix years ago, vifibly from weight and longevity, which was fuppofed to have been about 200 years. There had been many other names of eftimation handed down to us, though the realities are now totally worn out, and have ceafed to exift. Can any better proof be defired, that engrafted fruits are not permanent, then the regret we feel for the lofs of thefe old valuable fruits.

Facts refpecting engrafted fruits, their degree of permanence, &c.

To make my paper as fhort as convenient, I have dwelt only on the apple and pear; yet all the engrafted fruits are under the fame predicament of the feed not producing its like, and the offspring in time falling into a nothingnefs of growth and bearing, though that fpace of time muft certainly depend on the natural longevity and hardnefs of the fort, foil, pofition, care, &c. All thefe are more fully expreffed in the papers publifhed in the different volumes of the Tranfactions of this Society, and the two volumes of the Orchardift, wherein the whole fyftem is extended, to form a rational culture for the management of Standard Fruits.

It fhould be remembered, that as I am now alluding to the ftate of actual permanency, fifty years are to be accounted as nothing; and as often as we come to that point, we are compelled to refort to our firft affertion, "That engrafted fruits are not permanent, they being continued from elongations, and not raifed as a repetition of feeds." This is the only

rational

Facts respecting engrafted fruits, their degree of permanence, &c.

rational way as yet introduced of accounting for the lofs of the valuable old varieties of fruits. Should a better fyftem be introduced, I fhall readily adopt it; but this fufficiently anfwers the purpofes of the planter.

Some years ago, from due inveftigation and thorough conviction, I propagated this principle; and it was publifhed in the 17th volume of the Society's Tranfactions, in the following words: "All the grafts taken from this firft tree, or parent ftock, or any of the defcendants, will for fome generations thrive; but when this firft ftock fhall, by mere dint of old-age, fall into actual decay, a nihility of vegetation—the defcendants, however young, or in whatever fituation they may be, will gradually decline; and, from that time, it would be imprudent, in point of profit, to attempt propagating that variety from any of them. This is the dogma which muft be received. I do not expect a direct affent, neither do I wifh it, for it fhould be taken with much referve; but it is undoubtedly true." Thefe confiderations fhould ftimulate us in fearching after new varieties, equal, or perhaps fuperior to thofe of which we regret the lofs.

Obferve that, from the time the kernel germinates for apple-quick, fhould the plant be difpofed to form a valuable variety, there will appear a regular progreffive change, or improvement, in the organization of the leaves, until that variety has ftood, and grown fufficient to bloffom and come into full bearing; that is, from the ftate of infancy to maturity; and it is this and other circumftances, by which the inquifitive eye is enabled to form the felection among thofe appearing likely to become valuable fruits. But from that time the new variety, or felected plant, compared with all the engraftments which may be taken from it, or any of them, thefe fhall fhew a moft undeviating famenefs among themfelves.

It is readily allowed, that the different varieties of fruits are eafily diftinguifhed from each other by many particulars, not only refpecting their general fertility, and the form, fize, fhape, and flavour of the fruit, but alfo the manner of the growth of the tree, the thicknefs and proportion of the twigs, their fhooting from their parent ftem, the form, colour, and confiftence of the leaf, and many other circumftances, by which the variety can be identified; and were it poffible to engraft

engraft each variety upon the fame ftock, they would ftill retain their difcriminating qualities, with the moft undeviating certainty.

Facts refpecting engrafted fruits, their degree of permanence, &c.

The proper conclufion to be drawn from the ftatement in the laft paragraph, is this—that were any one to put the thought in practice on a full-grown hardy or crab ftock, it would produce an excellent proof that engrafted fruits are not permanent. For if twenty different varieties were placed together, fo that each might receive its nurture from the fame ftem, they would gradually die off in actual fucceffion, according to the age or ftate of health of the refpective variety, at the time the fcions were placed in the ftock; and a difcriminating eye, ufed to this bufinefs, would nearly be able to foretell the order in which each fcion would actually decline. Should it alfo happen that two or three fuckers from the wilding ftock had been permitted to grow among the *twenty grafts*, fuch fuckers or wilding fhoots will continue, and make a tree after all the reft are gone. A further confequence would refult from the experiment: among fuch a number of varieties, each of the free growers would ftarve the delicate, and drive them out of exiftence only fo much the fooner. It muft be obferved, that this fuppofed ftem is the fofter-parent to the twenty fcions, and real parent to the fuckers; and thofe the leaft converfant with engrafted fruits know the advantage acquired from this circumftance. And here it is worth while remarking, that a Gafcoyne, or wild cherry, will grow to twice the fize that ever an engrafted cherry did.

By an experiment we have had in hand for five years, it will appear that the roots and ftem of a large tree, after the firft fet of fcions are exhaufted or worn-out, may carry another fet for many years; and we fufpect a third fet, provided the engrafting is properly done, and the engrafter choofes a new variety. Now the Ripfton pippin, of Yorkfhire, is the favourite, as being a free grower and good bearer, with fine fruit. This however may be certainly depended on, that when a new apple is raifed from feed, if a fcion were placed in a retired fituation, and conftantly cut down, as a ftool in a copfe-wood, and the apple never fuffered to fulfil the intentions of nature in bearing fruit, the practitioners of the following ages may fecure fcions from that ftool, to continue the variety much longer. Hence, though I have written as much as is in

my

Facts respecting engrafted fruits, their degree of permanence, &c.

my power againſt permanency, yet I have taken ſome pains to aſſure the planters, that forecaſt, ſelection, pruning, cleanlineſs, and care, will make the orchards turn to more profit for the riſing generations, than what they have done for the laſt hundred years.

To place the nature of varieties in its true light, for the information of the public, I muſt maintain, that the different varieties of the apple will, after a certain time, decline, and actually die away, and each variety, or all of the ſame ſtem or family, will loſe their exiſtence in vegetation; and yet it is a known fact, and mentioned in the 17th volume of the Tranſactions, that after the debility of age has actually taken poſſeſſion of any variety, it will yet thrive by being placed againſt a ſouthern wall, and treated as wall-fruit. Who, however, can afford to raiſe cider at that expenſe, except as matter of curioſity, to prove, that when the vital principle in vegetation is nearly exhauſted, a ſuperior care and warmth will ſtill keep the variety in exiſtence ſome time longer?

It ſhould be underſtood, that the external air of Britain is rather too cold for the delicate fruits, which is the reaſon why, in the Orchardiſt, I lay ſuch a ſtreſs on procuring *warmth* for the trees, by *draining*, *ſhelter*, and *manure*. It would be now loſt time to attempt to recover the old varieties as an article of profit.

If I have not expreſſed myſelf, in this Eſſay on the Nature of Varieties, with ſo much clearneſs and conviction as might have been expected, it ſhould be conſidered that it is an abſtruſe ſubject, very little underſtood, and requiring at firſt ſome degree of *faith*, *obſervation*, and *perſeverance*. The prejudices of mankind revolt againſt it. They are not diſpoſed to allow the diſtinction of nature; and they imagine, that in the act of engrafting or multiplying they give new life, whereas it is only continuing the exiſtence of the ſame tree, ſtick, or bud. Obſerve what I ſaid before:—the ſeed of the apple, when placed in the earth, germinates, and unfolds itſelf into a new plant, which ſucceſſively paſſes through the ſtages of infancy, maturity, and decay, like its predeceſſors. I might ſay, all created nature is ſimilar in this reſpect; though, from the circumſtance that varieties are much longer-lived than man, the plants have appeared to be poſſeſſed of eternal

eternal powers of duration: nothing fublunary, however, which poffeffes either animal or vegetable life, is exempt from age and death.

Facts refpecting engrafted fruits, their degree of permanence, &c.

Within the laft twenty years I have travelled many hundred miles, and converfed with the moft intelligent men in each country; and I now want to convince mankind, for no other reafon than becaufe it is their intereft fo to believe, that there is in creation an order of beings (engrafted fruits) fo formed, that we have the power of multiplying a fingle variety, to whatever number of trees we pleafe;—that the firft fet arifes from a fmall feed;—that the next and defcendent fets are propagated by engraftings, or from cuttings, layers, &c.;—and that although thefe trees may amount to millions, yet, on the death of the primogenious or parent ftock, merely from old-age, or nihility of growth, each individual fhall decline, in whatever country they may be, or however endued with youth and health. I fay they fhall gradually begin to decline; and in the courfe of time, or of centuries, to thofe who would prefer that expreffion, the *whole variety* will fcarcely have a fingle tree remaining to fhow what the fruit was. Let thofe who are not difpofed to affent to this ftatement, afk themfelves what is become of the old loft varieties? did they die, or did wicked men malicioufly cut them up?

I, who am firmly convinced of the truth of what I have advanced on this fubject, have no doubt but that the fame would happen by engrafting on the Oak or Beech, if the maft raifed from the engrafted tree did not produce the like; for there the queftion turns.

Is it not known, that the woodman, in fetting out his fapling oaks, always felects new feedling plants, and never continues one upon an old ftool; and that if he fhould fo blunder, that tree, from the ftool, will neither have the freedom of growth, nor the fize or firmnefs of timber, equal to a new-raifed plant.

I wifh I could perfuade my friends, that, with the fame attention with which the woodman acts, the planter is to raife his orchard from the young fruits which thrive in the neighbourhood, or are in health and full bearing in the country whence they are to be brought.

The fruit-grower fhould look to felection, cleanlinefs, and care. To me it is a circumftance perfectly indifferent, whe-

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Facts refpecting engrafted fruits, their degree of permanence, &c.

ther he is to ufe Mr. Forfyth's compofition, Mr. Bulingham's boiled linfeed oil, or my medication. I only maintain that the wounded parts of trees want fomething to deftroy the infects and vermin, and heal the wood, from which the trees are kept in health.

Let thofe who are bleffed with fruit-plantations attend to their prefervation, and not leave them to the ftate of unaffifted nature.

I am, Sir,
Your moft obedient Servant,
THO. SKIP DYOT BUCKNALL.

*Hampton-Court,*
12*th Oct.* 1801.

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## XIII.

*Explanation of the Subjects concerning which Queftions were propofed at Page* 71 *of the prefent Volume; namely,* 1. *The Place of the Erect Image behind a Concave Speculum;* 2. *Of the Image formed by a Concavo Convex Mirror, which is not Left-handed, and has the Property of revolving along with the Mirror; and* 3. *The Figure of the Sky. By* W. N.

Anfwers to certain queftions.

AS the queftions of my correfpondent R. B. have not been treated by any other of my friends, I fhall here difcufs them according to my promife.

Erect image in a concave mirror.

1. The queftion refpecting the erect image in the concave mirror relates to a phenomenon which formed the fubject of difcuffion in print very early in the laft century, though I cannot at this moment recollect the authors who have related the facts and arguments. R. B. is perfectly correct as to the various divergence of the rays compofing the pencils which iffue from radiant points at different diftances from the eye, but it does not appear that our notions of diftance are greatly regulated by that circumftance. Our judgment of the diftances of near objects is partly founded on the adjuftment required to be made in the eye itfelf to produce diftinct vifion by the rays of the feveral pencils, as R. B. remarks, but much more on a circumftance commonly noticed by authors, but by him overlooked, namely, the degree of convergence between the optical axes of the two eyes which is required to avoid fquinting, or

Our judgment of diftance is not founded on the nature of the pencils of light;

or the phenomenon of two images. In proof of this, if he will take two pens or pencils or other pointed objects, and, ſhutting one eye, endeavour to apply their points end-wiſe to each other by moving them acroſs the line of ſight, he will find how much leſs preciſely one eye can give a judgment on diſtances, than both eyes. And our judgment of diſtant objects is eſtabliſhed not at all upon the nature of the pencils, but on the perſpective arrangement or angular magnitude of the objects together with their gradual obſcuration (from the interpoſed maſs of air) called aerial perſpective. Hence we find that the ſeveral parts of a well drawn ſketch of mere lines, and ſtill more of an excellent painting, give all the notions of diſtance, though there cannot be any difference in the divergence of the pencils of light to correſpond with that effect. The conſciouſneſs that a body is approaching or retiring ariſes little if at all from any change in the pencils; in near objects that notion is rather gained from the required change in the optical axes, and in theſe and all diſtant objects it is almoſt entirely produced by the angular magnitude becoming larger or ſmaller, while we contemplate the appearances.

but on the ſituation of the eye, and the perſpective.

Theſe truths will explain the image in the concave mirror: For 1. When the face is near the mirror, the image is ſeen by a great convergence of the optical axes, which becomes leſs and leſs as the obſerver retires, at the ſame time that the image itſelf ſubtends a leſs and leſs angle:—It therefore, for both reaſons, appears to retire. 2. At a greater diſtance the image ſeems ſtationary while the obſerver retires, becauſe the angular enlargement of the image is nearly compenſated by its diminution from increaſed diſtance; and 3. At a ſtill greater diſtance the angular enlargement increaſes ſo faſt that the image ſeems rapidly to come forward until it is loſt in confuſion.—If both eyes be kept open, the obſerver has a conſiderably accurate notion of the real diſtance of the frame, which, together with the confuſion of two different images preſented to the eyes, may lead him to have the ſentiment of a mere angular enlargement; but if one eye be covered, the notions of retreat, ſtationary poſition, and rapid advance of the image will infallibly be adopted.

The image in a concave mirror ſeems to retire and advance from theſe cauſes.

2. With regard to the concavo-convex mirror, none of the pencils of light are brought truly to a focus. If we conceive the ſurface of the mirror to be divided into zones by lines

Explanation of the effects of a concavo-convex mirror.

drawn perpendicularly acrofs the fhorteft arc of convexity that paffes through the vertex; and thefe lines to be indefinitely near each other;—or to fpeak lefs mathematically, if the mirror were divided into fmall parallel ftripes, drawn right acrofs its concavity;—each of thofe fmall zones or ftripes will reflect the light of any diftant object to a focus before the mirror without any fenfible error; whence it will diverge in a kind of flat pencil; and thefe pencils themfelves will not be parallel to each other, but will diverge from a point behind the mirror or virtual focus; fo that this laft divergence of the pencils will be at right angles to the divergence of the rays that compofe them. If therefore we attend to the portion of light which enters the eye from what may be taken to be a radiant point of the image, in this cafe, we fhall perceive that they cannot be brought to a correct focus on the retina. For if the eye be adjufted to the place of convergence before the mirror, the focal fpot will be elongated in the direction of the arc of convexity; and on the contrary, if the eye be adjufted to the virtual focus of the convex, or point of divergence, the rays of the pencils will not be duly collected, but will render the fpot on the retina oblong in the direction of the arc of concavity. Neverthelefs, if the diftance of the eye be fo great, as that the difference of adjuftment for thefe two points fhall not produce any fenfible effect of this kind, the whole image will appear fufficiently neat and diftinct, though a little deformed; that is to fay, thofe dimenfions which are governed by the concavity, will be fomewhat greater than thofe governed by the convexity; becaufe as we may affirm, the concave mirror is nearer the eye than the convex. From this general explanation, it will be eafily underftood that the image muft difappear and become utterly confufed by the quantity of crofs aberation, when any attempt is made to examine it with a convex lens; and that it will be impoffible to form an image of the fun or of any other object upon the furface of paper or other fimilar material. In fact the reflected light from the fun confidered as a luminous point, will converge before the mirror to a line or cauftic curve of which I have not yet confidered the properties; but which, if the concave mirror were cylindrical, would be a right line at the diftance of half the radius from the vertex.

It is not neceffary after what has been faid to enter into any fuller explanation of the reafon why the image is inverted, (that is to fay either right handed or upfide-down) with regard

to

to the relative poſition of its parts conſidered acroſs the concavity, and the contrary along the lines of convexity;—or, while theſe lines have their poſition altered by rotation of the mirror, the image itſelf will alſo appear to revolve with twice the velocity. Theſe effects, as well as that of the extreme diſtortion of the image, ſeen when the object is within the centre of concavity, will be eaſily apprehended by the reader, whoſe notions of optics are ſufficient to enable him to underſtand the preceding paragraph.

Why the ſky appears flat, and the ſun and moon large in the horizon.

3. The enlarged appearance of the ſun and moon at low altitudes or near the horizon, and the apparent flatneſs of the concavity of the ſky have been explained, as my correſpondent obſerves, by reference to the diminiſhed light of the heavenly bodies, when it paſſes through a long portion of the atmoſphere, and likewiſe to the notions of diſtance which are obtained by looking over a long row of terreſtrial objects. That is to ſay the explanation is grounded on the ſuppoſition or notion of greater diſtance along the horizontal line deduced from conſideration of the aerial and the geometrical perſpective. The obſervations and objections of R. B. appear to me to be perfectly well founded. I think the effect is produced almoſt entirely by the geometrical perſpective, not of the houſes, trees, and other land objects, but of the clouds themſelves. When the clouds are in low diſtinct flakes with clear openings between them, the angular magnitude both of the clouds and their intervals will be greater near the zenith, and will diminiſh as the zenith diſtance encreaſes, ſo as on ſome occaſions to exhibit the appearance of objects running out to an immenſe diſtance in the concavity of an extremely flat dome. On the contrary, when lofty towering maſſes of clouds riſe from the horizon to a conſiderable elevation facing the ſetting ſun, or otherwiſe ſo circumſtanced that the tint of the whole maſs ſhall be very little varied, the mind, ſo far from adopting the notion of an extended dome in that part, ſhall receive the impreſſion of an immenſely elevated wall, with very little curvature as it riſes. Both theſe conditions are not unfrequently to be ſeen at one and the ſame time, and the ſky is then, according to the obſervation of R. B. very far from exhibiting a figure of regular dimenſions. A great variety of intermediate forms and groups of the clouds often preſent themſelves, by which the apparent figure of the ſky will be made to differ from theſe extreme caſes. When the ſky is perfectly and cloſely clouded, we

have

have perhaps very little notion of an apparent figure unlefs from our former habits, or from the angular motion produced by the wind. This will be greateft near the zenith, and by fhewing us that the clouds over our heads are nearer than the others, will give us a conviction of the figure of a flat arch.

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*Ufeful Notices refpecting various Objects. By a Correfpondent.*

*Cotton in Ink.* R. B.

Cotton in ink recommended.

THE ancient practice of putting cotton in ink is almoft entirely given up. But when we confider that the colouring matter of ink is merely a precipitate, in the act of flow fubfidence, and that the gum as well as the Gallic acid, are fubject to fpeedy decompofition or mouldinefs by expofure to the air, we fhall fee good reafons for refuming it. The black fecula is kept fufpended by the cotton; the fluid is prevented from circulating by heat, cold or mechanical agitation, and confequently prefents a very fmall furface to the air; and laftly the method of dipping tends to keep the pen clean and in good condition. I can take upon me to affert that ink in cotton is blacker, more durable, and much lefs liable to become mouldy. The latter effect may be almoft entirely prevented by occafionally turning the cotton upfide down, that is to fay, every two or three days

*Shaving with Water.*

Shaving with water.

IN fome of your early numbers the operation of fhaving has been rather amply difcuffed by yourfelf and correfpondents. I have nothing to add in the way of reafoning to what has been there brought forward; but I take this opportunity of noticing a fact. Long ago it was obferved to me that the foap anfwers no other purpofe than that of clearing the fkin, in proof of which it was afferted that the face fo cleaned may be fhaved quite as well without the lather as with it. I made trial of this procefs, but did not find it fucceed, becaufe I wiped the fkin dry. But I have fince found that if the beard be well wafhed with foap, and then with clear water, the operation of fhaving may be very effectually and pleafantly performed, while the water continues upon the fkin. The effects are that the razor cuts more keenly and clofely, and the fkin feems to be much lefs tender. I leave your other correfpondents to reafon and difcover the caufe of the excellence of this method; in the mean time I fhall continue to ufe it, unlefs a ftill more confiderable improvement fhould be made.

Digester in which the Heat, and pressure, are rendered stationary. by Sir. A. N. Edelcrantz.

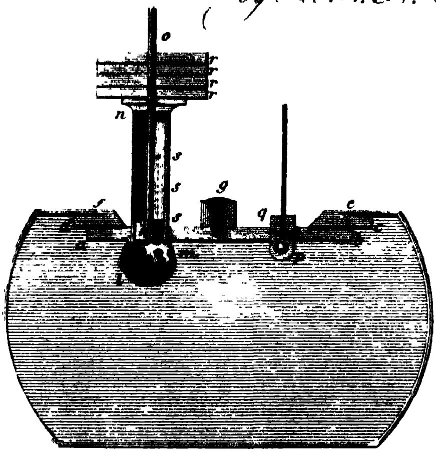



Fig. 1.

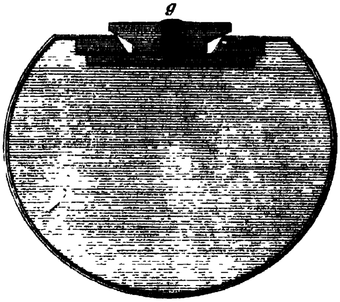



Fig. 2.

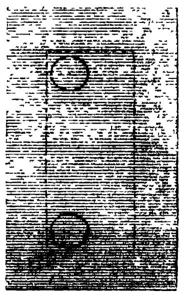


Fig. 3.


Fig. 4

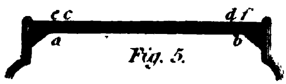
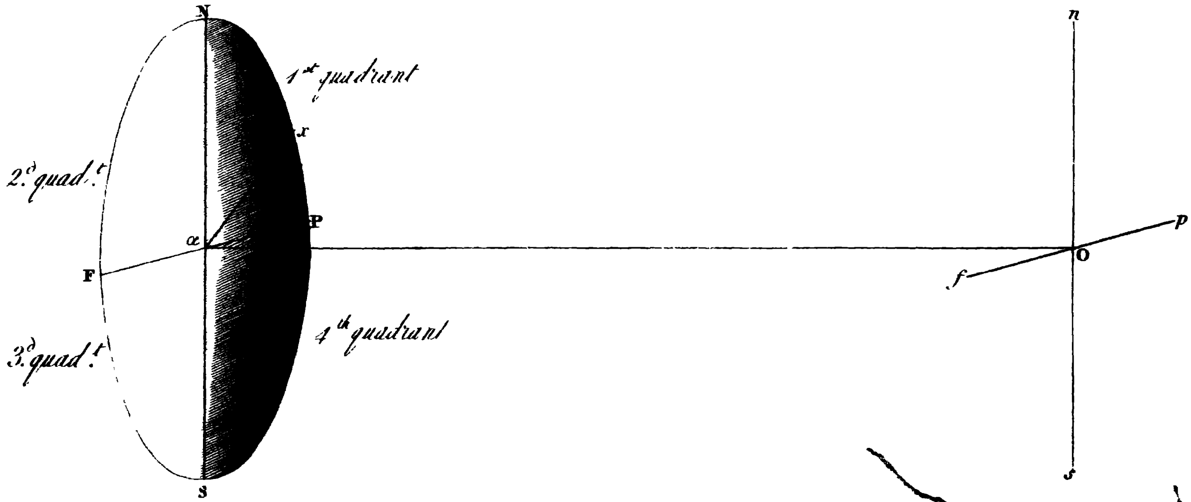


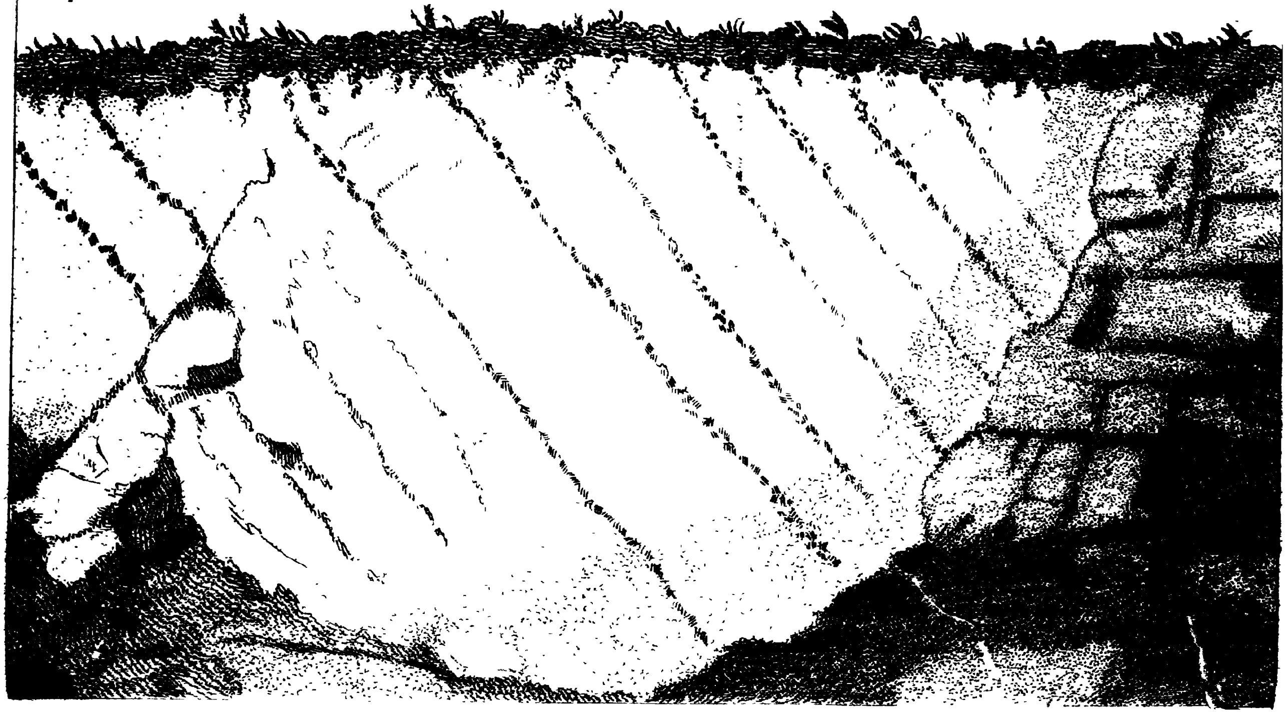

Fig. 5.

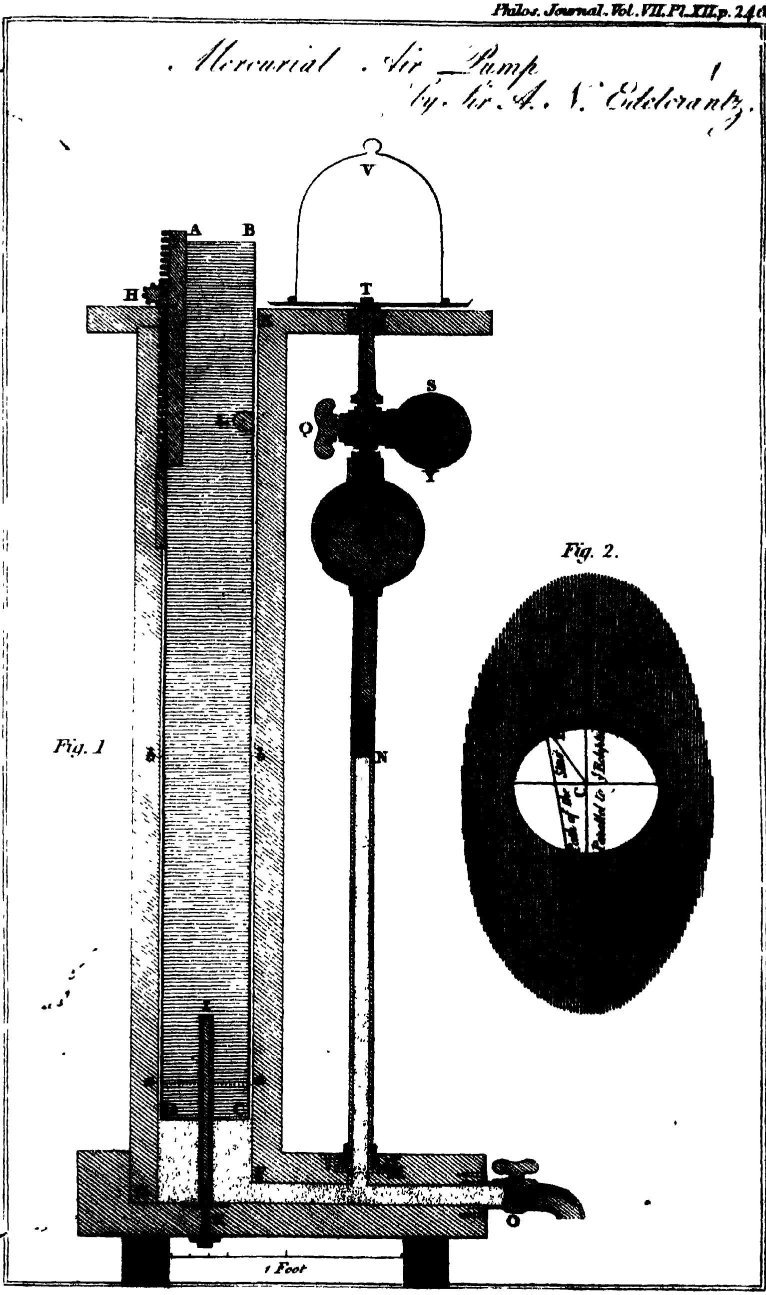
Drawn by Blunt. Engraved by Mutlow.

*Dr. Edwards theory of the Changes in the situation of double Stars.*



Remarkable Strata of Flint.




Mercurial Air Pump
by Mr A. N. Edelcrantz.
V
A
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T
S
Q
Fig. 1
N
Fig. 2.
C
O
1 Foot

A
# JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

*APRIL*, 1804.

## ARTICLE I.

*Experiment proving the Advantage of Perifcopic Spectacles. Communicated in a Letter from* W. H. WOLLASTON, *M. D. F. R. S.*

To Mr. NICHOLSON.

SIR,

THE opinion given by Mr. Jones, in your laft Journal, refpecting the improved form of fpectacle-glaffes, on which I had delivered my fentiments in the number preceding (p. 144,) induces me to addrefs you once more upon that fubject. Mr. Jones's opinion of the improved fpectacle glaffes,

It appears wholly unneceffary to follow Mr. J. through his various obfervations, or to remark upon the experiment by which he deceives himfelf; becaufe all doubt of the advantage of the perifcopic glaffes may be removed by the following direct comparative trial, which any perfon who choofes can make without difficulty. referred to experiment.

I have before me two glaffes, each of four inches "pofitive focus," as propofed by Mr. J. the one double-convex, which in his judgment is pronounced to be "*indubitably the beft and moft convenient that can be devifed*" (p. 194); the other a concavo-convex, or menifcus, which he thinks "*evidently the worft of the two for a fpectacle-glafs,*" (p. 197.) With the form of lens recommended by Mr. Jones,

When I fix the former at the diftance for moft diftinct vifion oppofite to a printed octavo page, and approach my eye to the glafs, the extent of diftinct vifion compared with that by the

perifcopic glafs was as 24 to 40,

glafs, I cannot without pain read quite twenty-four lines; but upon fubftituting the perifcopic glafs fixed in the fame pofition I can with eafe difcern every word in the page, which contains forty lines.

—or rather as 1 to 3.

The enlargement of the field of view obfervable in this trial is fufficient to evince the fuperior utility of the perifcopic glafs; but were there occafion to compare more nearly the circular areas that may be feen with equal diftinctnefs, they would be found to differ by a ratio as great as that of three to one.

A correfponding advantage in fhallower lenfes.

This difference is of courfe more evident in glaffes of fo high power than in thofe generally ufed by long-fighted perfons for common purpofes; but it cannot be doubted, that a correfponding, though fmaller, inequality fubfifts wherever there is the fame diffimilarity of conftruction, even when the focal diftance is longeft.

Conclufion.

The advantage in queftion is therefore indifputably proved by direct experiment; to the *novelty* of it Mr. J. himfelf has unintentionally contributed very fatisfactory evidence; but as to its importance, thofe alone, who have the misfortune to labour under any defect of vifion, muft ultimately decide.

I remain, Sir,

Your obliged humble fervant,

*March* 20, 1804. W. H. WOLLASTON.

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## II.

*Additional Obfervations on fome remarkable Strata of Flint in the Ifle of Wight, in a Letter from Sir* CHARLES ENGLEFIELD, *Bart. F. R. S. to* JOHN LATHAM, *M. D. F. R. S. and L. S.** *(See Page 183 of the prefent Number.)*

DEAR SIR,

Obfervations on remarkable ftrata of flint in the Ifle of Wight.

I FEEL much flattered at the notice taken of my Paper on the chalk pits in the Ifle of Wight by the Linnean Society; and as I wifh to render my account of the very curious appearances obferved by me in them as perfect as I can, the following additional obfervations on the fubject made during a fecond vifit laft year are, by your favour, fubmitted to the Society.

* Linnean Tranfactions, Vol. VI.

The

The pits I laſt year inſpected are as follow, beginning from the eaſt:

Obſervations on remarkable ſtrata of flint in the Iſle of Wight.

Brading pit, which is at the eaſtern point of the great ridge, where the valley of Brading-haven interſects it, and ſeparates it from the Yaverland-hill, which terminates in the ſea at Culver and Bembridge. A road cut into the chalk above Knighton. Aſhley-down pit, about three miles eaſt of Newport. A pit very near to and ſouth of Cariſbrook caſtle. The cliffs and caves of Freſhwater bay, both eaſt and weſt of the valley which interſects them entirely and runs from Freſhwater to Yarmouth.

The Yaverland chalk is therefore the only part which I have not examined; and little doubt can be entertained of its ſimilarity to the reſt of the range, to which it evidently belongs.

In Brading pits ſome flints appear in detached nodules, and theſe are ſound and unbroken.

The inclined ſtrata of flint are viſible, but not to advantage, owing to the manner of working the pits. In theſe ſtrata the flints are univerſally ſhattered, ſome into abſolute powder, others into groſſer powder and fragments mixed. But beſides theſe ſtrata, the chalk in this pit is divided by vaſt perpendicular fiſſures, as ſmooth as plaiſter walls, and in ſome of theſe fiſſures flint has formed, which appears broken like that in the ſtrata.

The road above Knighton only juſt cuts into the chalk ſtratum, but all the flints viſible in the banks are extremely ſhattered.

The pit at the weſt end of Aſhley-down, near two large barrows, is the moſt extenſive and ſatisfactory of any I have ſeen. The perpendicular face of the chalk, where worked, is not leſs than fifty or ſixty feet, and its direction is at right angles to that of the ſtrata, and parallel to their line of dip:—of courſe they are ſeen to very great advantage. The ſtrata ſeems to dip northward more rapidly than in any other place where I could obſerve them. The angle of inclination is from 75 to 80 degrees. There are not layers of flints between every layer of the chalk. Some of the chalk is peculiarly ſolid, and riſes in very large maſſes affecting a cubic form. Their ſolid vein is from twenty-five to thirty feet thick, and is in ſtrata from three to four feet. In all this ſolid part there are very few flints.

Both above and below this harder bed (ſpeaking of the original poſition of the ſtrata) the chalk is ſofter, and has more

 flints

Obfervations on remarkable ftrata of flint in the Ifle of Wight.

flints in it. The ftratified flints in this pit are full as much fhattered as any I had feen. The nodules are not at all broken. Many of the ftratified flints are much defaced in this pit by an admixture of pyrites, fo as to be quite opaque like a coarfe jafper, and thefe flints are much fofter than the others, as is always the cafe in the impure flints.

In the chalk pits near Carifbrook the ftrata are not fo vifible as in the pits north of the caftle (defcribed in the firft papers) but the flints are to the full as finely, though perhaps not fo generally broken. In one flint I obferved that though it lay in its bed undifturbed, chalk as if in a fluid ftate had run into one of the fiffures. Every appearance of this pit indicates that the chalk, fince its ftratification, has received a moft violent fhock.

The chalk at Frefhwater bay appears in high perpendicular cliffs, particularly on the weftern fide of the bay. Both on the eaft and weft the ftrata dip northward near 80 degrees, and the dip feems to run eaft and weft very regularly. The weftern cliff has a very regular and perpendicular face to the eaftward; and here the parallel direction of the ftrata, each feparated by a thin line of black flint prefents a moft curious appearance. The flint here is often found in thin plates of confiderable extent, fometimes not above an inch thick, and feems formed from each fide of the fpace which it fills, as the exterior parts (or thofe neareft the chalk) are the pureft and blackeft, and it is gradually whiter towards the middle, where there is often a foft line of chalk included between the two plates of flint. All the ftratified flints are more or lefs fhattered, and fome are reduced to very fine powder. The cave at Frefhwater, which is really a beautiful as well as a very curious one, is formed by the action of the fea on thefe nearly vertical ftrata. They are of different hardnefs, and all interfected with fiffures at right angles to the ftrata. When the fea acts on and wears away a foft ftratum, a gallery is formed, and the upper parts of the ftratum between fiffure and fiffure drop out, much in the fame way as bricks are apt to do out of the flat arch over a window, the harder contiguous ftrata ferve as walls to the gallery, but are by degrees perforated in different parts, and become irregular pillars, fupporting the vaft weight of the hills above, until the action of the fea weakens them fo far that they fall, and a part of the face of the hill goes with them, fo that the cave is conftantly, although flowly, changing its form.

Larger maſſes of the harder ſtrata, defended by their flint coating, alſo ſtand up in the bay as inſulated rocks of different ſhapes, and much reſembling the Needle Rocks, which are exactly of the materials and formed by the ſame proceſs. In this part of the chalk ſtratum, I ſaw ſeveral foſſils remain which I had ſought in vain in the pits I had viſited. One was ſingular, it had the appearance of part of a very large ſhell, regularly ſtriated, and almoſt flat. I have often ſeen ſmall fragments apparently of a ſimilar ſhell in chalk, but never a large piece. It was ſo firmly fixed as not to be removed without a chiſſel, which I had not.

Obſervations on remarkable ſtrata of flint in the Iſle of Wight.

To theſe obſervations on the chalk of the iſland, I muſt add, that this whole range, although really chalk, is much harder than the chalk of the South-downs, inſomuch that the carpenters cannot uſe it for drawing lines, but import chalk for that purpoſe from Portſdown hill above Portſmouth. They alſo call the iſland chalk by the name of marle, which is, however only the Britiſh name for chalk, and appears in many compound names, ſuch as Marl-borough on the Wiltſhire chalk hills, and the very ſignificant one of Albemarle, or white chalk.

As I have made ſome further obſervations on the ſouthern range of hills which form the back of the iſland, I will treſpaſs on your patience a little longer, particularly as they in ſome degree contradict, or rather correct, what I had advanced on that ſubject in my former letter.

When the northern front of thoſe hills is viewed from Aſhley down, the ſtratum of ſtone mentioned in my former letter as lying directly under the chalk of St. Catharine's and Dunnoſe hills, appears every where to maintain an horizontal poſition; and ſo in its general poſition, particularly in its northern front, it certainly does, but juſt behind the village of Ventnor, the ſtratum entirely diſappears, as if it had been ingulphed in a great chaſm, and a deep and narrow valley runs winding into the chalk hill of St. Boniface, though it does not penetrate through it, which ſeems the remains of the fiſſure into which the ſtone had ſunk.

The appearances of the great ſtone ſtratum, from Niton eaſtward to Ventnor are noted as follows in the journal made on the ſpot.

On an attentive inſpection of the ſtrata of the under cliff, it appears that the great ſtratum of rugged and laminated ſtone,

which

Obfervations on remarkable ftrata of flint in the Ifle of Wight.

which firft appears at the weft fide of St. Catharine's, and thence ranging eaftward, forms the front of the cliff, over-hanging the Underway dips in its fouthern face gently to the eaftward. The cliffs at Mirables are much higher above the fea than thofe at St. Laurence, and from thence they decline, till at the opening of the hill above Ventnor they totally dif-appear. A fmall crag juft peeps out of the eaftern face of this dell, and the whole hill of St. Boniface is, as far as can be feen, compofed of chalk. As however this, like all other chalk hills, is in the ftate of a fteep flope, covered with turf, perhaps by giving into its face the ftony ftratum might be difcovered. It is alfo to be obferved that the chalk, which is not vifible above the rock at Mirables, begins to appear foon after, and grows gradually thicker as it proceeds eaftward. At St. Laurence it forms a thick cap to the rocks; and at Steephill Shute, its thick-nefs is very much encreafed, and foon after nothing but chalk appears in St. Boniface's hill. In what form the rock re-ap-pears at Dunnofe to the eaft of St. Boniface, I have had no opportunity of examining.

I fhould not, dear Sir, trouble you with thefe defective ob-fervations, but that every notice, however imperfect, may be of ufe when connected by future obfervations, and that they may ferve as a ftimulus to other travellers, who often go over this beautiful line of country, to turn their attention to its fin-gular natural phenomena.

I fend you two fpecimens of the broken flints, one from Brading, the other from near Carifbrook, but the tickets are miflaid, and I am not fure which is which,

I remain, &c.

*Tilney-Street, May* 26, 1801.

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## III.

*Account of a Machine for fweeping Chimnies, by a Blaft of Air. In a Letter from Mr.* J. C. HORNBLOWER, *Engineer.*

To Mr. NICHOLSON,

DEAR SIR,

Introduction.

I SHALL be much obliged if you will allow me a page or two in your Journal for a defcription of a machine I have lately got up for the purpofe of cleanfing chimnies from foot. My

wifh is, if poffible, to keep alive the fpark that has been kindled by a truly philanthropic individual, with a view to reftore a degraded clafs of our fpecies to their rank in civil fociety. The machine, as it is conftructed folely for the purpofe mentioned above, is certainly novel in its intention, and without a better acquaintance with its principle and operation than the public are at prefent in poffeffion of, it is not likely to gain that notice which in my judgment it is entitled to. There is a propenfity in us to turn into ridicule any thing that will admit of a ridiculous conftruction, or afford an opportunity for the difplay of our wit; and fuch is the unfortunate circumftance of this machine, that grinning takes precedence of gravity the moment it is announced as a candidate for the gold medal at the Adelphi. However, happy for it, and thrice happy for me, there is a tribunal which can judge fairly and fcientifically of its merits, for which I prefent it as follows.

Propenfity to ridicule inventions.

The body of the machine A, *Plate* XIII. is made of copper, of about 3lb. to the fquare foot, and its contents are about three cylindrical feet. In the middle of the cover, which is foldered on, is a fyringe or condenfer, having its handle as appears above the cover. On one fide of the cover is inferted a crooked pipe, having a valve opening inward in its inner or lower end; the ftem of which comes up through the pipe, and terminates with a button C. The pipe is continued by a flexible leather one, to which is united the tin plate tube B B.

Defcription of the apparatus. A veffel, into which air is condenfed, communicates with a tube charged with fmall gravel, which being blown up the chimney brings down the foot.

This tube B B has a crofs bar of tin plate fixed edge-ways in the bottom or breech of the tube which detaches from the leather pipe fomewhat like a piftol barrel, leaving a piece of tube about fix inches remaining to the leather pipe, into the bottom of which this cafe bar is fixed, and ferves to receive a charge of fmall gravel, having a piece of paper firft laid on the crofs bar; the other part of the tube is then to be replaced, and the air-veffel fuppofed to be full; the valve is preffed down by a little lever accompanying the machine, and its contents are difcharged into the fhaft of the chimney; and if there is any foot worth fweeping away, it will come down.

*Obfervations concerning the Machine.*

I do not pretend to have made any new difcovery in the conftruction or application of the machine, for it is well known that a mufket has often been applied for the fame purpofe, and with

This operation is fimilar to firing a mufket up the chimney.

with as good effect as need be, barring the hazard of setting fire to the soot; and indeed, when a chimney has accidentally taken fire, it is the best and most expeditious mode of putting it out, at least of bringing the soot into a situation by which it may be extinguished.

Estimate of the velocity of the extended air, and its effect in the chimney; together with that of the gravel.

I have said that the vessel contains nearly three cylindrical feet, and we cannot crowd three atmospheres of air into it, in which case there will be 42 lbs. per inch square at round numbers, pressing against the charge in the tube, or rather against the valve, the tube being $2\frac{1}{4}$ inch diameter, which amounts to 168 lbs. for the whole area. This air will all be discharged in one second, the mean velocity of which may be fairly reckoned at 50 feet, in that time having impetus in itself sufficient to carry away any soot of consequence in a chimney of 100 feet high; but when we take into the account the charge of gravel, being alternately incident and reflected on all sides of the chimney, we need not fear to affirm that it is perfectly applicable to its intention.

Additional tubes for curved chimnies.

To render it as universal as possible, there must be another tube, to be occasionally used, when the side of the chimney near the fire place is gathered over in order to bring the throat of the flue over the fire, as at *Fig.* II. from *a* to *b*, or it should be in several pieces, to conform to the height of this gathering, and in this case the charge must be at the upper fissure at *b*, and the pipe stayed as perpendicularly as can be conveniently done; for which purpose there is a piece made to fix on the pipe, having two stems, as *Fig.* III. which if put in the bearing side of the pipe, will keep it upright and in the crater of the flue. The machine thus constructed is to be laid on a little truck, and transported from house to house, with a gallon of gravel to begin with; because until it has obtained the good opinion of the public, it would not be necessary to attempt such regulations as would afterwards be deemed requisite to facilitate the operation.

Trial at the house of the Society of Arts in the Adelphi.

This poor thing, brought forth under other disadvantages besides its singularity, was conducted to the Adelphi, to undergo an eventful trial, in competition with some others, who had no such singularity to be imputed to them; they being all descendents of the stock of one family, the remotest relation between each not being more distant than cousin-german.—

Other machines also exhibited there,

One of them however possessed great originality and ingenuity in

in the defign. It confifted of a congeries of brufhes, which in preparation for action were collapfed by their conftruction fo as to admit of being thruft up the chimney with very little refiftance, and when it had gained the top, on pulling a ftring, it expanded to the dimenfions of the chimney, and brought the foot down before it; but from want of a mechanical attention to its conftruction, fome parts being too ftrong, and fome too flight, it broke in the firft experiment. Brufhes.

Another (very elegant inftrument) was prefented to the committee, confifting of a large brufh, fuch as I have feen in fome churches to fweep down the cobwebs, with a fcraper attached to it; lengthened to the height of the chimney by a feries of whalebone rods, whipped together as in the conftruction of waggoners' whips, very accurately jointed to each other. Indeed this was effential and common to them all, except one; and that was a brufh, not to be fent up the chimney, but down it, by getting on the top of the houfe, and putting it into the chimney, and fo letting it fall to the bottom. However, it was faid, they would all do very well but mine, as a machine like that was adopted to a rectilinear flue only; whereas theirs had fo many joints and other properties to conform to a curvature, that they would be fure to go through a crooked flue as well as a ftrait one. A fingle brufh. Notion that the author's machine is fuited only to a ftrait flue.

When the late Doctor Johnfon had any thing advanced to him of the marvellous kind, his way of hefitation to admit it, was by faying "It may be very true, but it is impoffible."— So I fay with refpect to the difpofition of thefe rods, when fet in competition with a volume of air going at the rate of fifty feet in a fecond from the lower part of the chimney to the top. What is there to ftop it? What is there to prevent its conformity to the curvature or *angles*, if you will? The fame denied.

The manner in which thefe brufhes act, let their conftruction be what it may, is by ftriking off the foot. What does a boy within the flue with the brufh in his hand? To be fure fweeping is the moft appropriate term for what he does; but then, unlefs, we attend to the peculiarity of the action, and confider how far it is poffible to effect that fweeping by another mode of action, we fhall not get any further than the boy in the flue. But is it not as eafy to conceive of a quantity of air being put into action along a road or a ftreet, fo as to fweep away the duft before it? No; you will fay, it blows it along; but To *blow* down the foot is as natural as to *fweep* it down.

but the term blowing affociates only with the accuftomed notion of the action of wind, and was the effect to be produced by the agency of fteam; the term blowing would more eafily be difmiffed, and we might call the action *driving* it along.

The term fweeping is in fome inftances ufed to exprefs the action of wind.

When we affociate this agency of the wind with its effect on a field of ripening corn, we conceive of it *fweeping* along or over the corn; and the fame effect would follow, were we to conceive of a large brufh of long hair carried on the tops of corn; fo that however popular expreffion, or the prejudice of cuftom may operate to diftinguifh the caufes producing the fame effect, it becomes us to waive fuch diftinctions, and admit the efficiency of the one as much as the other.

Inftances of the action of wind in a chimney.

I have obferved that when a door has been fuddenly opened or fhut, it has brought down foot in a chimney which has a fluggifh draft; or a piece of paper accidentally thrown on the fire has had the like effect; and this by a momentary acceleration of the current of air in the flue. What then will not fuch a machine as this perform, with fuch a volume of air, with fuch a velocity.

The air machine fweeps chimnies of all heights, and is durable.

One circumftance prefents itfelf, by which the air machine muft have a decided preference over the brufhes; which is this, you muft know the height of the chimney, and adapt the length of the rods to that height, or elfe you will not know when you are in the flue or out of it. Whereas my machine knows no neceffity for fuch a punctilio; all that is neceffary being only to give fo many fhocks of the condenfer to a chimney of two ftories, and fo many to one of three, and fo on. And again, thefe brufhes in the very outfet of trial made with them, are fubject to accidents, and will moft affuredly wear out very faft. What muft become of thofe whalebone rods when the fewing is rubbed through in paffing up and down againft the projections of unceremonious bricks and mortar? What repairs will they not be fubject to in the courfe of one day's action? Whereas the air machine will fweep a hundred chimnies, and be repaired for two-pence; it wanting only a little oil in the condenfer.

It was tried at the Adelphi upon chimneys that contained no foot.

When this machine was carried to the Adelphi, it came to be the third in courfe for the trial, and it was ordered *down, down* to the foundation of the houfe, for fear a chimney fhould not be found high enough in any other part of the building.—But when it was brought, behold there had been two fweepers already, which obliged me to apologize for the machine, by obferving

obferving that they had taken down all the foot, and left nothing for it to do *. Upon which a fuggeftion was made, that there was another chimney which had a fire alternately during the winter †; and on examination it was agreed, that, by the appearance of the foot about the breaft of the flue, the chimney was very foul, of which, however, I had my doubts. But it was thought expedient to try that chimney; and the firft charge we gave it brought no foot. We gave it another charge, but no foot came; and it was thought beft to fend up a boy, with proper implements, to fweep the chimney; but no foot came down, except what may be faid to be brought away by violence, and did not exceed a quart-potful, and with it much mortar or pargetting.

This brings me to fome obfervations I have made on the condition of chimnies in general, as to the foot they may contain.

Trials of the machine with other chimnies.

I tried this machine at Shoreditch Workhoufe, where the chimnies are very lofty, for which reafon I preferred them; but when I came to fee their circumftances I fufpected how it was, and we tried two of them where the fires had been very economical, and may continue fo winter after winter, without ever requiring to be fwept at all, for reafons I will give by and by. I then went to a neighbour of mine, and we fwept two of his chimnies; one had no foot, the other had a good deal; and we thought fit to get a boy to go up the laft chimney, and he brought down about a hatful of foot; however, my neighbour was fatisfied, and figned my certificate to the Committee at the Adelphi.

We then went to another neighbour's, and fwept his parlour chimney, and diflodged a great quantity of foot. We were to have fwept his kitchen chimney, but it having a fmoke-jack, we could not apply the machine for want of the additional pipe in *Fig*. 2.

* It appears the committee had no regard to the effect of any of the candidate's inventions, any further than a fair thruft up the chimney.

† With another in the fame room I fuppofe.

*Account*

## IV.

*Account of the Changes that have happened, during the last Twenty-five Years, in the relative Situation of Double Stars; with an Investigation of the Cause to which they are owing.* By WILLIAM HERSCHEL, *LL.D. F.R.S. From the Philosophical Transactions for* 1803.

*(Concluded from Page 227.)*

Obfervations and inferences refpecting the changes of relative fituation in ftars extremely near each other.

I SHALL now enter into a more detailed examination of the feveral angles of pofition I have taken at different times, and fhow that they agree perfectly well with the appearances which muft arife from the revolution of a fmall ftar round Caftor. A calculation of thefe angles may be had, by finding the annual motion of the fmall ftar, from the change of 21° 54′, which has been fhown to have taken place in 23 years and 142 days. Accordingly, I have given, in the firft column of the following table, the time when the angles were taken. In the fecond, are the angles as they were found by meafure; they are all in the north-preceding quadrant. The third column contains a calculation from the annual motion of 56′,18, obtained as before mentioned: it fhows what thefe angles fhould have been, according to our prefent fuppofition of a revolving ftar. And the laft column gives the difference between the obferved and calculated angles.

| Times of the obfervations. | Obferved angles. | Calculated angles. | Differences. |
|---|---|---|---|
| Nov. 5, 1779 - | 32° 47′ | 32° 47′ | 0° 0′ |
| Feb. 23, 1791 - | 22 57 | 22 11 | + 0 46 |
| Feb. 26, 1792 - | 27 16 | 21 16 | + 6 0 |
| Dec. 15, 1795 - | 13 52 | 17 42 | − 3 50 |
| March 26, 1800 | 18 8 | 13 41 | + 4 27 |
| April 23, 1800 | 10 30 | 13 37 | − 3 7 |
| Dec 31, 1801 - | 7 58 | 12 2 | − 4 4 |
| Jan. 10, 1802 - | 10 53 | 12 1 | − 1 8 |
| Jan. 23, 1802 - | 10 28 | 11 59 | − 1 31 |
| Feb. 28, 1802 - | 13 0 | 11 53 | + 1 7 |
| Feb. 11, 1803 - | 7 53 | 11 0 | − 3 7 |
| March 23, 1803 | 13 23 | 10 54 | + 2 29 |
| March 27, 1803 | 10 53 | 10 53 | 0 0 |

On

Obfervations and inferences refpecting the changes of relative fituation in ftars extremely near each other.

On looking over the fourth column of this table, it will be found, that the differences between the obferved and calculated angles are not greater than may be expected, confidering that moft of the early meafures are fingle, and cannot have the accuracy which may be obtained by repetition. Even as they are, we muft acknowledge them fufficient to afcertain the gradual change in the angle of pofition of the two ftars. In one place, the difference amounts to fix degrees; but it will foon appear, that a more accurate annual motion gives a calculated pofition which takes off much of the error of this meafure.

In a converfation with my highly efteemed friend the Aftronomer Royal, he happened fome time ago accidentally to mention, that Dr. Bradley had formerly obferved the two ftars of $\alpha$ Geminorum to ftand in the fame direction with Caftor and Pollux. It occurred to me immediately, that if the time of this obfervation could be nearly afcertained, it would be of the greateft importance to the fubject at prefent under confideration. For, fhould Dr. Bradley's pofition be very different from a calculated one, it would induce us at once to give up the idea of a revolving ftar. The obfervation was made by Dr. Bradley with a view to fee whether any change could be perceived in the courfe of the year, by which the annual parallax of the ftars might be difcovered. Dr. Mafkelyne, who had this information from Dr. Bradley in converfation, had made a memorandum of it in his papers. He has been fo kind as to look for it; and, as foon as he found the note, he fent me the following copy, which I have his permiffion to tranfcribe.

"*Double ftar Caftor. No change of pofition in the two ftars:* "*the line joining them, at all times of the year, parallel to the line* "*joining Caftor and Pollux in the heavens, feen by the naked* "*eye.*"

Dr. Mafkelyne informs me, that the obfervation muft have been made about the year 1759; and alfo mentions, that he himfelf verified the fact, as to the line joining the two ftars appearing through the telefcope parallel to the line joining Caftor and Pollux, in 1760 or 1761; but that he did not examine it at various times of the year.

The advantage of having an angle of pofition obferved in 1759 by Dr. Bradley, and fo foon after verified by Dr. Mafkelyne, will give us an addition of 20 years to our period. On calculating the right afcenfion and polar diftance of Caftor and Pollux

Obfervations and inferences refpecting the changes of relative fituation in ftars extremely near each other.

Pollux for November 5, 1759, it appears, that a line drawn from Pollux through Caftor, muft have made an angle of 56° 32′ north preceding with the parallel of that ftar; and, this being alfo the pofition of our double ftar, we have an interval of 43 years and 142 days, for a change of 45° 39′, from the time of Dr. Bradley's obfervation to that of my laft meafure of the angle. By this we are now enabled to correct our former calculation, which was founded upon a fuppofition that the firft angle of pofition I had taken was perfect; but this could hardly be expected, and on examination it appears that the meafure was 2° 40′ too little. The annual motion, by our increafed period, is 1° 3′,1; and the computation of the angles of pofition in the third column of the following table, as well as the differences contained in the fourth, are made according to this motion.

| Times of the obfervations. | Obferved angles. | Calculated angles. | Differences. |
|---|---|---|---|
| Nov. 5, 1759 - | 56° 32′ | 56° 32′ | 0° 0′ |
| Nov. 5, 1779 - | 32 47 | 35 29 | − 2 42 |
| Feb. 23, 1791 - | 22 57 | 23 36 | − 0 39 |
| Feb. 26, 1792 - | 27 16 | 22 32 | + 4 44 |
| Dec. 15, 1795 - | 13 52 | 18 32 | − 4 40 |
| March 26, 1800 | 18 8 | 14 3 | + 4 5 |
| April 23, 1800 | 10 30 | 13 58 | − 3 28 |
| Dec. 31, 1801 - | 7 58 | 12 12 | − 4 14 |
| Jan. 10, 1802 - | 10 53 | 12 10 | − 1 17 |
| Jan. 23, 1802 - | 10 28 | 12 7 | − 1 39 |
| Feb. 28, 1802 - | 13 0 | 12 1 | + 0 59 |
| Feb. 11, 1803 - | 7 53 | 11 1 | − 3 8 |
| March 23, 1803 | 13 23 | 10 54 | + 2 29 |
| March 27, 1803 | 10 53 | 10 53 | 0 0 |

When the refult of this table is compared with that of the former, it will be feen that my obfervations agree not only very well with Dr. Bradley's pofition, but even give more equally divided differences than before, fo that the excefs and differences counteract each other better than in the firft table.

The time of a periodical revolution may now be calculated from the arch of 45° 39′, which has been defcribed in 43 years and 142 days. The regularity of the motion gives us great reafon to conclude, that the orbit in which the fmall ftar moves about

about Caſtor, or rather, the orbits in which they both move round their common centre of gravity, are nearly circular, and at right angles to the line in which we ſee them. If this ſhould be nearly true, it follows, that the time of a whole apparent revolution of the ſmall ſtar round Caſtor, will be about 342 years and two months.

**Obſervations and inferences reſpecting the change of relative ſituation in ſtars extremely near each other.**

*γ Leonis.*

Our foregoing diſcuſſions will greatly abridge the arguments which may be uſed, to ſhew that this ſtar and its ſmall companion are alſo probably united in forming a binary ſyſtem. But, in order to give more clearneſs to our diſquiſition, we ſhall follow the arrangement which has been uſed with α Geminorum, and prefix the ſame letters to our paragraphs. Then, if any one article ſhould appear to be not ſufficiently explained, we need but turn back to our firſt double ſtar, where the ſame letter will point out what has already been ſaid more at large on the ſubject; and an application of it may eaſily be made.

The diſtance of the ſtars γ and $x$, as I ſhall again call the ſmall one, has undergone a viſible alteration in the laſt 21 years. The reſult of a great number of obſervations on the vacancy between the two ſtars, made with the magnifying powers of 278, 460, 657, 840, 932, 1504, 2010, 2589, 3168, 4294, 5489, and 6652, is, that with the ſtandard power and aperture of the 7-feet teleſcope, the interval in 1782 was $\frac{1}{4}$ of a diameter of the ſmall ſtar, and is now $\frac{3}{4}$. With the ſame teleſcope, and a power of 2010, it was formerly $\frac{1}{2}$ of a diameter of the ſmall ſtar. and is now full 1 diameter. In the years 1795, 1796, and 1798, the interval was found to have gradually increaſed; and all obſervations conſpire to prove, that the ſtars are now $\frac{1}{2}$ a diameter of the ſmall one farther aſunder than they were formerly. The proportion of the diameter of γ to that of $x$, I have, by many obſervations, eſtimated as 5 to 4.

The firſt meaſured angle in 1782, is 7° 37′ north following *; and the laſt, which has been lately taken, is 6° 21′ ſouth following.

* In my ſecond Catalogue of double Stars, (Phil. Tranſ. for 1785, page 48,) the angle of poſition is 5° 24′. This was taken April 18, 1783; and, not being acquainted with the motion of the ſmall ſtar, I ſuppoſed it to be more accurate than the former meaſure.

Obfervations and inferences refpecting the change of relative fituation in ftars extremely near each other.

lowing. The fum of thefe angles gives 13° 58′, for the change that has taken place in 21 years and 38 days. To account for this, we are to have recourfe, as before, to the various motions of the three bodies.

*Single Motions.*

(*a*) The motion of $x$ alone cannot be admitted, fince it is known that $\gamma$ Leonis is not at reft. The annual proper motion of this ftar, according to M. De la Lande, is $+ 0'',38$ in right afcenfion, and $0'',04$ in declination towards the fouth.

(*b*) $\gamma$ cannot be the only moving body; becaufe its motion in right afcenfion only, which, in 21,1 years, at the parallel of $\gamma$, amounts to $7'',49$, would have long ago taken it away from the fmall ftar.

(*c, d, e,*) The fun cannot be the only moving body; becaufe its motion in right afcenfion will not account for that of $\gamma$ Leonis, which ftar therefore cannot be at reft. And, if we were willing to give up the former affumed folar motion, in order to fix upon fuch a one as would explain the motion of $\gamma$, we fhould be under a neceffity to contradict the united evidence of the proper motions of many principal ftars which are in oppofition to it.

*Double Motions.*

(*f*) When two motions are propofed, we cannot fix upon $\gamma$ and $x$ for the moving bodies, unlefs we fhould fet afide the folar motion, and this, we know, cannot properly admit of a doubt.

(*g*) That we cannot allow O and $x$ to be the two bodies in motion, follows from the infufficiency of the folar motion to account for that of $\gamma$, which muft be real, or at leaft partly fo.

(*h*) If O and $\gamma$ are the moving bodies, the given fituations of $x$, in the years 1782 and 1783, point out an apparent motion of $x$, which muft be intirely owing to the folar parallax; and, therefore, thofe who will admit this hypothefis, muft grant the difcovery of the motion of the folar fyftem, and of the proportional parallax of the two ftars $\gamma$ and $x$. Let us however examine whether any motion of the fun, fuch as we can admit, will account for the change of pofition and diftance pointed out by my obfervations of the fmall ftar near $\gamma$ Leonis.

The

The joint effect of proper motion and parallax, has carried $\gamma$ from its situation in 1782 to that where we now find it. The small star, having all this time, in appearance, accompanied $\gamma$, must have gone through a space of 7″,98, in a direction which makes an angle of 8° 30′ south following with the parallel of $\gamma$, in order to be at its present distance from it, and at the same time to have undergone the required change of its angle of position. Now, as the supposition we are examining requires this small star to be actually at rest, it will be necessary to assign to the sun an opposite motion of the same velocity, in order to make that of $x$ only an apparent one. The consequence of this will be a retrograde motion of the sun, which it is well known cannot be admitted.

Observations and inferences respecting the change of relative situation in stars extremely near each other.

*Motion of the three Bodies.*

($i$) A motion of all the three bodies, is the only way left to explain the phenomena of our double star; and I shall now again point out the very particular circumstances which it is requisite should all happen together, to produce the intended effect.

Let the motion of the sun, with the same annual velocity 1, as in the case of $\alpha$ Geminorum, be directed towards $\lambda$ Herculis. Then the effect of this motion will shew itself at the place of $\gamma$ Leonis, in the annual velocity of ,3314, and in a direction which makes an angle of 31° 11′ south preceding with the parallel of that star. In this calculation, I have admitted the distance of the largest of the two stars of $\gamma$ from the sun to be 3, that of $\alpha$ Geminorum being 2. But, if any other distance should hereafter be considered as more probable, the calculation may be easily adapted to it. The consequence of the parallax thus produced on $\gamma$ Leonis in 21,1 years, will be an apparent motion of 2″,788 south preceding, in the abovementioned direction; and, on $x$, it will be in the same time, and in the same direction, 1″,091. As the small star must not be too near $\gamma$, we have, in the calculation, supposed it to be at the distance of 4 from O.

The real annual proper motion of $\gamma$ is required to be 3,5202; and its direction must make an angle of 3° 40′ north following with the parallel. By this motion alone, $\gamma$ would have passed over a space of 9″87 in 21,1 years; but, when it is combined with the apparent motion arising from parallax, the star will come into its present situation.

Obfervations and inferences refpecting the changes of relative fituation in ftars extremely near each other.

The real annual motion of $x$ muft be 4,6294, in a direction 0° 20′ fouth following. This will carry it over 9″,74, in 21,1 years; and, when combined with the apparent motion which the folar parallax will occafion, both together will bring it to its proper diftance from $\gamma$ Leonis, and to a fituation which will agree with the laft obferved angle of pofition.

From what has been faid, it is again evident, that not only as many particular circumftances muft concur in explaining the phenomena of $\gamma$ Leonis as we have pointed out with $\alpha$ Geminorum, but that a very marked condition is added in our fecond double ftar, which requires an adjuftment of velocities in $\gamma$ and $x$, which fhall alfo fit the fame folar motion that was ufed in $\alpha$ Geminorum. And this proves, that every additional double ftar which requires the fame condition in order to have its ap pearances explained, will inforce the arguments which have been ufed, in a compound ratio.

If, on the other hand, we have recourfe to the fimplicity of the known effects of attraction, and admit the two ftars of our prefent double ftar to be united in one fyftem, all the foregoing difficulties of accounting for the obferved phenomena will vanifh. Whatever may be the proper motion of the fun, the parallax arifing from that caufe will affect both ftars equally, on account of their equal diftance from the fun. The proper motion of $\gamma$ Leonis alfo may be in any direction, and of any given velocity, fuch as will agree beft with aftronomical obfervations; fince the motion of a fyftem of bodies will not interfere with the particular motion of the bodies that belong to it, fo that our fecondary ftar will continue its revolution round the primary one without difturbance.

It will now be neceffary to examine the obferved angles of pofition, and to compare them with calculated ones; but, as there has been a change in the diftance of the two ftars, it is evident that, if they revolve in circular orbits, the fituation of the plane of their revolution muft be confiderably inclined to the line in which we fee the principal ftar.

Let N F S P, Fig. 2, be the orbit in which $x$ revolves about $\gamma$ placed in the centre. Suppofe a perpendicular to be erected at $\gamma$ leading to O, not expreffed in the figure. By an obfervation of Feb. 16, 1782, we have the angle F $\gamma$ $x$ = 7° 37′ north following; and the proportion of the apparent diameter of $\gamma$ to that of $x$ has been given as 5 to 4. It has alfo been afcertained

tained, that the vacancy between the apparent diameters, when the first angle of position was taken, was $\frac{1}{4}$ diameter of the small star; and the last angle of position being 6° 21′ south following, with a distance between the stars of $\frac{1}{4}$ diameter of the small star, we obtain the two points or centres of the small stars $x x'$, through which an ellipsis $a b x x' c d$ may be drawn about γ. This will be the apparent orbit in which the small star will be seen to move about γ, by an eye placed at O. And the inclination of the orbit to the line in which we see the double star, will be had sufficiently accurate to enable us to give a calculation of the several angles of position that have been taken. The ellipsis we have delineated shews that the small star, in its first situation $x$, could not be much past its conjunction at $b$, and that, consequently, in passing from $x$ to $x'$, the parts of the apparent elliptical arch, which are projections of the real circular arch $h h'$, would be described in times nearly proportional to the time in which the whole arch has been described. Upon these principles, the third column of the following table has been calculated.

Observations and inferences respecting the changes of relative situation in stars extremely near each other.

| Times of the observations. | Observed angles. | Calculated angles. | Differences. |
|---|---|---|---|
| Feb. 16, 1782 - | 7° 37′ nf | 7° 37′ | 0° 0′ |
| April 18, 1783 - | 5 24 nf | 6 51 | − 1 27 |
| Jan. 24, 1800 - | 3 16 sf | 4 15 | − 0 59 |
| Feb. 19, 1800 - | 3 23 sf | 4 18 | − 0 55 |
| March 26, 1800 | 3 47 sf | 4 22 | − 0 35 |
| Jan. 26, 1802 - | 6 4 sf | 5 35 | + 0 29 |
| Feb. 10, 1803 - | 3 33 sf | 6 16 | − 2 43 |
| March 22, 1803 | 6 32 sf | 6 20 | + 0 12 |
| March 26, 1803 | 6 21 sf | 6 21 | 0 0 |

The difference between the calculated and observed angles, contained in the fourth column of the preceding table, is so little, that we may look upon the gradual change of these angles as established by observation; and we may form a calculated estimate of the time which will be taken up by the mutual revolution of the two stars. The apparent places $x x'$, being referred to their real ones, give the arch $h h'$, which has been described in 21 years and 38 days; and this arch, seen from the centre γ, is about 6° 20′: it follows, that the length of a whole revolution of our small star round γ Leonis, will be about 1200 years.

*ε Bootis.*

Obfervations and infeiences refpecting the changes of relative fituation in ftars extremely near each other.

This beautiful double ftar, on account of the different colours of the ftars of which it is compofed, has much the appearance of a planet and its fatellite, both fhining with innate but differently coloured light.

There has been a very gradual change in the diftance of the two ftars; and the refult of more than 120 obfervations, with different powers, is, that with the ftandard magnifier, 460, and the aperture of 6,3 inches, the vacancy between the two ftars, in the year 1781, was $1\frac{1}{2}$ diameter of the large ftar, and that it now is $1\frac{3}{4}$. By fome earlier obfervations, the vacancy was found to be confiderably lefs in 1779 and 1780; but the 7-feet mirror then in ufe was not fo perfect as it fhould have been, for the purpofe of fuch delicate obfervations. By many eftimations of the apparent fize of the ftars, I have fixed the proportion of the diameter of ε to that of $x$, as 3 to 2. Auguft 31, 1780, the firft angle of pofition meafured 32° 19′ north preceding *; and, March 16, 1803, I found it 44° 52′, alfo north preceding: the motion, therefore, in 22 years and 207 days, is 12° 33′. It fhould alfo be noticed, that while the apparent motion of α Gemmorum, and of γ Leonis, is retrograde, that of ε Bootis is direct.

A proper motion in this ftar, if it has any, is ftill unknown; our former arguments, therefore, cannot be applied to it, without fome additional confiderations; and, as many others of my double ftars will ftand in the fame predicament, I fhall give an outline of what may be faid, to fhow that this, and probably many of the reft, are alfo binary fyftems.

*Single Motions.*

(*a*—*e*) If ε Bootis is a ftar in which no proper motion can be perceived, we may infer, from the highly probable motion of the folar fyftem, that this ftar, which is of the third magnitude, and on that account within the reach of parallax, muft have a real motion, to keep up with the fun, in order to prevent an

* The angle of pofition, in my firft Catalogue of double Stars, Phil. Tranf. for 1782, page 115, is 31° 34′ (it fhould be 54) north preceding. This will be found to be a mean of the three firft meafures hereafter given in a table of pofitions.

apparent

apparent change of place, which muſt otherwiſe have happened. In this caſe, no ſingle motion can be admitted to explain the phenomena of our double ſtar. But, if a real proper motion of ε Bootis ſhould hereafter be aſcertained, the arguments we have uſed in the caſe of γ Leonis, will lead to the ſame concluſion.

Obſervations and inferences reſpecting the change of relative ſituation in ſtars extremely near each other.

*Double Motions.*

(*f*) ε and *x* cannot be the moving bodies; and our former argument (*f*) will apply to every double ſtar whatſoever.

(*g*) O and *x* cannot be alone in motion; for, if no motion in ε can be perceived, it muſt move in a ſimilar manner with the ſun, and none of the three bodies will be at reſt. But, if its proper motion ſhall hereafter be found out, it muſt either be exactly the reverſe of the ſolar motion, and therefore only an apparent one, or it will be more or leſs different. In the latter caſe, all the three bodies muſt be in motion; in the former, the exact quantity of the ſolar motion will be diſcovered, and the relative parallax of many ſtars may be had by obſervation.

(*h*) If O and ε are the two bodies in motion, and if at the ſame time no motion in ε can be perceived, then the apparent motion of *x* muſt be intirely owing to the different effect of the ſolar parallax on ε and *x*; but the effect of the ſolar parallax on *x*, can only be in a direction contrary to the motion of the ſun, which, being north following the ſmall ſtar, whether it be nearer or farther from us than ε, muſt have an apparent motion towards the ſouth preceding part of the heavens. But this is directly in oppoſition to my obſervation of the motion of the ſmall ſtar, which, theſe laſt 23 years, has been directed towards the north following.

*Motion of the three Bodies.*

(*i*) Let the motion of the ſun be again towards λ Herculis; then, if no motion in ε Bootis be perceivable, it muſt move exactly like O. Highly improbable as it is, let it be admitted.

Then, in addition to this extraordinary ſuppoſition, a third motion is alſo required for *x*, which, aided by the ſolar parallax, is to carry it likewiſe within a quarter of a diameter of ε, into the ſame place where, though unperceived, the large ſtar has been carried by its own motion; that is, in order to be appa-

rently

Obfervations and inferences refpecting the change of relative fituation in ftars extremely near each other.

rently at reft, the fun, ε Bootis, and its fmall companion, muft all move exactly alike, fetting afide the very little difference in the pofition and diftance of the fmall ftar, which, in the whole, amounts to little more than 6-tenths of a fecond; than which, certainly nothing can be more improbable.

But, if ε fhall hereafter be found not to have been at reft during the time of my obfervations upon it, then its place will be given; and, fince alfo the fituation of $x$, with refpect to ε, is to be had from my angles of pofition and diftances of the two ftars, the cafe will be fimilar to that which has already been confidered, in the paragraph (*i*), under the head of γ Leonis.

I may here add a remark with regard to ε Bootis, which will be applicable to feveral more of my double ftars. In the milky-way, a multitude of fmall ftars are profufely fcattered, and their arrangement is very different from what we perceive in thofe parts of the heavens which are at a confiderable diftance from it. About ε Bootis, which is fituated in what I have formerly called figuratively a nebulous part of the heavens *, there are, comparatively fpeaking, hardly any ftars; and, that fo remarkable a ftar as ε fhould have a companion, feems almoft to amount to a proof that this very companion is, as it appears to be, a connected ftar. The *onus probandi*, therefore, ought in juftice to fall to the fhare of thofe who would deny the truth of what we may call a fact; and I believe the utmoft they could do, would be to prove that we may be deceived; but they cannot fhow that this ftar has no connection with ε Bootis.

This argument will be much fupported, when we confider that many of the double ftars in the milky-way are probably fuch as have one of the fcattered ftars, nearly in the fame line, at a great diftance behind them. In this cafe, the two ftars of the double ftar have no connection with each other; and the great number of them in the milky-way, is itfelf an indication of this effect of the fcattered multitude of fmall ftars. In the fingle conftellation of Orion, for inftance, we have no lefs than 43, pointed out by my catalogues; ten of which are of the firft clafs, and yet have undergone no change of diftance or pofition fince I firft perceived them. But, with apparently infulated ftars, fuch as ε Bootis, the cafe is juft the reverfe.

* See Phil. Tranf. for 1784, page 449.

If,

If, in confequence of our former arguments, and the prefent remarks, we place ε Bootis among the ftars which hold a fmaller one in combination, we may delineate its orbit as in Plate VIII. Fig. 3. Obfervations and inferences refpecting the change of relative fituation in ftars extremely near each other.

Let P N F S reprefent a circle, projected into the elliptical orbit *a x x′ b c d*. ε is the large ftar; and *x x′* are the firft and laft meafured north preceding fituations of the fmall one, as given in the following table.

| Times of the obfervations. | Obferved angles. | Calculated angles. | Differences. |
|---|---|---|---|
| Auguft 31, 1780 | 32° 19′ | 33° 58′ | − 1° 39′ |
| March 13, 1781 | 30 21 | 34 13 | − 3 52 |
| May 10, 1781 - | 33 1 | 34 18 | − 1 17 |
| Feb. 17, 1782 - | 38 26 | 34 40 | + 3 46 |
| Auguft 18, 1796 | 45 32 | 41 40 | + 3 52 |
| Jan. 28, 1802 - | 49 18 | 44 19 | + 4 59 |
| Auguft 31, 1802 | 46 47 | 44 36 | + 2 11 |
| March 23, 1803 | 43 43 | 44 52 | − 1 9 |
| March 26, 1803 | 44 52 | 44 52 | 0 0 |

The real motion from *h* to *h′* is projected into that from *x* to *x′*; and, while the elliptical arch fubtends an angle of 12° 33′, the circular one will be about 4° 50′.

From the figure of the orbit, we may conclude that the fmall ftar, in its firft pofition, at *x* or *h*, was not more than between 30 and 40 years paft its conjunction; and that, confequently, the parts of the arch *x x′*, were nearly proportional to the times of their being defcribed. The pofitions have been calculated upon this principle; but with fome allowance for the firft obferved angle, which I fuppofe to have been a little too fmall; and, though the differences of the obferved and calculated angles are pretty confiderable, the obfervations are ftill fufficiently confiftent to prove the gradual change of the fituation of the fmall ftar.

The quantity of the change in 22 years and 207 days, will fhow that a periodical revolution cannot take up lefs than 1681 years. The real figure and fituation of the orbit, with many other particulars, are ftill unknown; it is, therefore, unneceffary to point out the uncertainties in which the inveftigation of the periodical time of the fmall ftar about ε Bootis muft long remain involved.

*ζ Herculis.*

### $\zeta$ *Herculis.*

Obfervations and inferences refpecting the changes of relative fituation in ftars extremely near each other.

My obfervations of this ftar furnifh us with a phenomenon which is new in aftronomy; it is, the occultation of one ftar by another. This epoch, whatever be the caufe of it, will be equally remarkable, whether owing to folar parallax, proper motion, or motion in an orbit whofe plane is nearly coincident with the vifual ray. My firft view of this ftar, as being double, was July 18, 1782. With 460, the ftars were then ½ diameter of the fmall ftar afunder. The large ftar is of a beautiful bluifh white, and the fmall one afh-coloured.

July 21, of the fame year, I meafured the angle of pofition, 20° 42′ north following. With the ftandard power, the diftance of the ftars remained as before. With 987, they were one full diameter of the fmall one afunder.

In the year 1795, I found it difficult to perceive the fmall ftar; however, in October of the fame year, I faw it plainly double, with 460; and its pofition was north following.

Other bufinefs prevented my attending to this ftar till the year 1802, when I could no longer perceive the fmall ftar. Sometimes, however, I fufpected it to be ftill partly vifible; and, in September of the fame year, with 460, the night being very clear, the apparent difk of $\zeta$ Herculis feemed to be a little lengthened one way. With the ten-feet telefcope, and a power of 600, I faw the two ftars of $\eta$ Coronæ very diftinctly; and, having in this manner proved the inftrument to act well, I directed it to $\zeta$ Herculis, and found it to have the appearance of a lengthened, or rather wedge-formed ftar; after which, I took a meafure of the pofition of the wedge.

Our temperature is feldom uniform enough to permit the ufe of very high powers; however, on the 11th of April, 1803, I examined the apparent difk, with a magnifier of 2140, and found it, as before, a little diftorted; but there could not be more than about ⅛ of the apparent diameter of the fmall ftar wanting to a complete occultation. Moft probably, the path of the motion is not quite central; if fo, the difk will remain a little diftorted, during the whole time of the conjunction. Our prefent obfervations cannot determine which of the ftars is at the greateft diftance; but this will occafion no difference in the appearance; for, if the fmall ftar fhould be the neareft, its light will be equally loft in the brightnefs of the large one.

The

The obfervations I have made on this ftar, are not fufficient to direct us in the inveftigation of the nature of the motion by which this change is occafioned. Obfervations and inferences refpecting the changes of relative fituation in ftars extremely near each other.

We may however be certain, that with regard to

*Single Motions.*

*(a, b)* Neither $x$ nor $\zeta$ can be fuppofed to be the only moving bodies, without contradicting the highly probable arguments for the fun's motion.

*(c, d)* If we admit the fun to be the moving body, the ftars $\zeta$ and $x$ being at reft, we may calculate the effect of the folar parallax upon them, as follows. Let O move towards $\lambda$ Herculis, with the annual velocity 1, as in the cafe of $\alpha$ Geminorum; then, from the fituation and magnitude of the large ftar of $\zeta$ Herculis, which we will fuppofe 4m, the effect of the folar motion at $\zeta$ will be only ,0522; and, at $x$, fuppofed to be at the diftance 5m, it will be ,0418. This will fhow itfelf at the parallel of $\zeta$ in a direction of 25° 5′ north preceding, the folar motion being in the oppofite direction fouth following. But this parallax will only produce, in 20 years and 10 months, an apparent change of 0″,444 in $\zeta$, and of 0″,355 in $x$; and will feparate the ftars, inftead of bringing them to a conjunction.

*(e)* A confiderable advantage may be gained, by placing $x$ at a little more than $\frac{1}{3}$ the diftance of $\zeta$ from O. For as, in the above-mentioned time, this would make the effect of parallax upon it 1″,18, a conjunction fhould now take place. But then the ftars, though very near each other, would not be quite in contact; much lefs could one of them occafion an occultation of the other. The fuppofition alfo, that the fmall ftar fhould be only $\frac{1}{3}$ of the diftance of the large one from us, is not very favourable to the hypothefis.

*$\delta$ Serpentis.*

This double ftar has undergone a very confiderable change in the angle of pofition, but none in the diftance of the two ftars. The 5th of September, 1782, an accurate meafure of the pofition was 42° 48′ fouth preceding; and February 7, 1802, it meafured 61° 27′ fouth preceding. In 19 years and 155 days, therefore, the fmall ftar has moved, in a retrograde order, over an arch of 18° 39′.

Every

Obfervations and inferences refpecting the changes of relative fituation in ftars extremely near each other.

Every argument, to examine the caufe of this motion, which has been ufed with ε Bootis, in the paragraphs from (a) to (j), will completely apply to this ftar; from this we may conclude, that the moft natural way of accounting for the obferved changes, is to admit the two ftars to form a binary fyftem. In this cafe we calculate, with confiderable probability, that the periodical time of a revolution of the fmall ftar round δ Serpentis, muft be about 375 years.

*γ Virginis.*

This double ftar, which has long been known to aftronomers,* has undergone a vifible change fince the year 1780, when I firft began my obfervations of it. The 21ft of November, 1781, I meafured the pofition of the two ftars, which was 40° 44′ fouth following. The ftars are fo nearly equal, that I have but lately afcertained the following one to be rather larger than its companion; the pofition, therefore, ought now to be called north preceding. By a mean of three meafures, that were taken on the 15th of April, 1803, the angle was 30° 20′ *np*.

The diftance, as far as eftimations by the diameter can determine, when the ftars are fo far afunder as thefe are, remains without alteration. May 21, 1781, they were 2½ diameters afunder; and, by eftimations lately made, with the fame inftrument and power as were ufed 21 years ago, the ftars are ftill at the fame diftance of 2½ diameters.

A very fmall proper motion in declination, of 0″,02 towards the fouth, has been affigned to this double ftar;† but the quantity is hardly fufficient for us to rely much upon the accuracy of the determination. I fhall therefore rather confider γ Virginis as one of the ftars of which we have no proper motion afcertained; and the arguments to which I fhall refer, will confequently be thofe which have been given with ε Bootis.

The change of the angle of pofition, in the time of 21 years and 145 days, amounts to 10° 24′; from which we obtain the annual motion of 29′,16. The obferved and calculated angles, with their differences, on which it will not be neceffary to make any remarks, are in the following Table.

* *Memoires de l'Academie des Sciences.* Ann. 1720.

† *Connoiffance des Temps*, Année VI. page 213.

Times

| Times of the Obſervations. | Obſerved angles. | Calculated angles. | Differences. |
|---|---|---|---|
| Nov. 21, 1781 - | 40° 44′ | 40° 44′ | 0° 0′ |
| Jan. 29, 1802 - | 28 22 | 30 51 | —2 29 |
| April 15, 1803 - | 30 20 | 30 20 | 0 0 |
| May 28, 1803 - | 32 2 | 30 17 | +1 45 |

Obſervations and inferences reſpecting the changes of relative ſituation in ſtars extremely near each other.

As a confirmation of the accuracy of theſe obſervations, we may have recourſe to a poſition of the ſame ſtars, deduced from the places of them, as they are given in Mayer's Zodiacal Catalogue. By two obſervations reduced to the beginning of the year 1756, the preceding one was 3″,8 before the other in right aſcenſion, and 5″,3 more north than that ſtar. From this we calculate the poſition, which was 54° 21′ 37″ north preceding. The interval from the 1ſt of January, 1756, to the 21ſt of November, 1781, is 25 years and 325 days. When this is added to the period I have given, we have 47 years and 105 days, for a motion of 24° 2′. The annual motion, deduced from this lengthened period, which is 30′,5, differs leſs than 1½ minute from that which has been calculated from my obſervations. With the aſſiſtance, therefore, of Mayer's obſervations, which greatly ſupports our calculation, we may conclude, that the two ſtars of γ Virginis revolve round each other in about 708 years.

## V.

*Account of the Invention of the Compound-Barrel and Winch, and its uſeful Application on a Large Scale. In a Letter from Mr.* J. J. HAWKINS.

To Mr. NICHOLSON.

SIR,

IN No. 25 of your uſeful Journal, p. 50, you mention the compound-barrel and winch, as the invention of the celebrated Mr. George Eckhardt.

Not in the leaſt wiſhing to ſubtract from the well known inventive talents of that Gentleman, I beg leave to ſay, that, if it was invented by him, it was alſo invented to my certain knowledge, by the late Mr. Robert Mc Kean of Philadelphia, ſon to the preſent governor of Pennſylvania.

The compound barrel and winch was alſo invented by Mr. R. Mc Kean.

 Mr.

In 1795 or 1796. Description.

Mr. Mc Kean communicated the idea to me a few days after it firſt occurred to him, in the year 1795 or 1796; his plan differed, however, in two points, from that deſcribed in your Journal; for, firſtly, he made uſe of handſpikes applied into holes in the end of the largeſt barrel, whenever the winch did not afford him ſufficient power; and, ſecondly, when he found it neceſſary to raiſe any thing above the level of his windlaſs, he added two other pullies for the rope to paſs over.

Dimenſions and application of a compound barrel.

About the year 1797, Mr. Mc Kean erected a ſaw-mill worked by a ſteam engine, at Bordenton in New Jerſey; where he employed this crane to raiſe his logs, from the ground to the frame on which they were to be cut, being a height of about twelve feet; the circumference of the largeſt barrel was about ſix feet, and its length three feet; the ſmalleſt barrel was of the ſame length, and its circumference three feet, making the difference of their circumferences three feet; and, conſequently one turn of the barrels, raiſed the log one foot ſix inches; and eight turns only were ſufficient, to elevate it the height required.

Its power as one to twenty.

This windlaſs was turned with handſpikes ſix feet long; which, at the place where the power is generally applied, would deſcribe a circle of about 30 feet; ſo that the power obtained, is as one to twenty. The length of rope neceſſary, including that which paſſes round the pullies, and from thence to the barrels, is about 55 feet.

A common tackle would have required five times as much rope;

Now in order to raiſe a weight 12 feet high, with a power of one to twenty, by means of the common block pullies, no leſs than 270 feet of rope is neceſſary; there is, however, this difference in the ſizes of the ropes, that, in the former

but thinner.

caſe ſupporting one half of the weight to be raiſed, whereas in the latter it has only to ſuſtain one twentieth part of it.

This windlaſs does not run down.

The windlaſs has the peculiar property of holding the weight at any part of its riſe or fall, without needing a ratchet-wheel and catch; by reaſon of the two parts of the rope pulling on oppoſite ſides of the barrels; and although one pulls on a longer lever than the other, yet it is not ſufficient to overcome the friction on the axis, which muſt of neceſſity be large, in order to give the requiſite ſtrength to the machine.

It promiſes to be extenſively uſeful.

I have lately introduced a ſmall windlaſs of this kind, into a piece of machinery requiring alternate elevation and depreſſion,

preffion, and find it extremely convenient; and I have no doubt it will be adopted in a great variety of machinery when its valuable properties are generally known.

I am,

Sir, Yours, &c.

J. J. HAWKINS.

11, *New Lifle Street,*
*March* 17, 1804.

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## VI.

*Propofal for conftructing a Galvanic Apparatus of Great Power, for the Combuftion of Metals. By a Correfpondent.* I. R. I.

To Mr. NICHOLSON.

SIR,

THE following idea of a mode of increafing to an immenfe degree the galvanic power of inflaming metals, took its origin from Mr. Wilkinfon's paper on that fubject in your laft Journal; and I beg that if you think this communication can be of the fmalleft ufe to that ingenious gentleman, that you will fend it to him before publication. Increafe of galvanic combuftion.

I can hardly think that it could have efcaped his penetration, (though I cannot account for his omitting the obfervation) that the natural confequence of his difcovery relative to the ratio by which the galvanic trough increafes in its power of inflaming metals, is, that the maximum of effect will be produced (from a given furface,) by one pair only; fo that the fame furface of metal that in his apparatus at prefent conftructing and diftributed into 50 pairs, will burn 108 feet of fteel wire $\frac{1}{70}$ of an inch thick, if concentrated into one, will have the fame effect on 5400 feet of the fame wire; an enormous increafe of a power that may become as noble a prefent to the arts, from fcience, as the fteam-engine has proved. by one fingle plate or pair.

I have alfo turned my thoughts towards making a new form of apparatus, on the above principle, fo as to condenfe it into as little bulk as poffible. The fimple infpection of the drawing accompanying this will fufficiently illuftrate it; the fcale is of three inches to a foot, (*Pl.* XV.) it reprefents part of the ground plan of a trough, of about ten feet long, two feet broad, and two and half feet deep: the black line reprefents the copper, the dotted the Defcription of the propofed apparatus.

the zinc, the white the acid liquor, and the shaded part the wood: the scale is made large for the sake of distinctness; two spigots or turncocks are adapted to the end by which the liquor may be drawn off; a contrivance of great use, but impracticable in the present form; only two convolutions of the plates are expressed, and the turns are made about three inches wide, to facilitate the soldering of the zinc plates to the copper; the liquor is confined in as small a space as possible, to prevent the unnecessary waste of acid. It may also be constructed with one metal only, on Mr. Davy's plan; in that case, the sulphuret will occupy one side of the plate, and the acid the other. Hoping that the above may merit a place in your very excellent Journal, and be of use to my fellow labourers in science,

I remain, Sir,

Your most obedient humble Servant,

I. R. I. an old Correspondent.

*Edinburgh, March* 13, 1801.

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## VII.

*A Memoir concerning the Fascinating Faculty which has been ascribed to the Rattle-Snake and other American Serpents. By* BENJAMIN SMITH BARTON, M. D *. *From the American Transactions,* Vol. IV.

FIDEM NON ABSTULIT ERROR.

Naturalists frequently too credulous.

NATURALISTS have not always been philosophers. The slight and superficial manner in which they have examined many

* Since this memoir was read before the Society, it has been considerably altered, and somewhat enlarged. I hope, the alterations will render it more worthy of the notice of those who, like myself, derive pleasure and happiness from the contemplation of the works and operations of nature, on this globe.

I fear, I shall be thought to have treated the question in too diffusive a manner. I have not, indeed, laboured to be concise. But if the memoir is more extensive than was necessary, I flatter myself, it will be admitted that it, at least, contains some new and interesting facts. I submit it to its fate. B. S. B.

' This interesting paper has been deferred from the pressure of other matter of more immediate and temporary claim. W. N.

many of the ſubjects of their ſcience; the credulity which has accompanied them in their reſearches after truth, and the precipitancy with which they have decided upon many queſtions of importance, are proofs of this aſſertion.

There is a queſtion in natural hiſtory that has, in an eſpecial manner, ſolicited from me theſe obſervations. I mean the queſtion concerning the faſcinating faculty, which has been aſcribed to different kinds of American ſerpents. It is my intention to examine this queſtion, in the memoir which I now preſent to the Philoſophical Society. Faſcination by ſnakes.

Of this faſcinating faculty we have all heard and read. In many of our country ſituations, there is hardly a man or a woman, who will not, when the ſubject comes to be mentioned, ſeriouſly relate ſome wonderful ſtory, as a convincing proof of the doctrine. Children ſeem taught to believe it. I think, it is ſometimes one of the earlieſt prejudices imprinted on their tender minds. It is a prejudice which often increaſes with their years; and even in that happy period of life when the mind is moſt firm, and the leaſt propenſe to the belief of extraordinary things, the ways of which we are not capable of ſcanning, I have known this prejudice ſo deeply and ſo powerfully rooted, as to mock the light and ſureneſs of facts, and all the ſtrength of reaſoning. generally believed in America.

It is not my intention, in this memoir, to give an analyſis, or complete view, of every thing that has been written on the ſubject. Nor is it my intention to examine the many ſtories, related by authors, in ſupport of the faſcinating faculty of ſerpents. For the firſt taſk, I have not leiſure; and, as to the ſecond, I ſhould think my time ill employed in pointing out the groſs abſurdities which ſeem to conſtitute a neceſſary part of many of thoſe ſtories. I think it proper, however, to obſerve, that I have anxiouſly ſought for, and have patiently peruſed, the volumes of tales publiſhed in favour of the doctrine which I mean to controvert. Examination of the queſtion propoſed.

I aim at giving a general, though correct, view of the queſtion, uninfluenced by the bold aſſertions of ignorance, or by the plauſible conjectures of ſcience. In the inveſtigation of the queſtion, I have ſought for facts: theſe have been my guides. I have ſtudiouſly endeavoured to follow where they ſeemed to lead. Perhaps, they have led me aſtray. General view.

The

The manner in which the fuppofed fafcinating power of ferpents is exerted has often been related, by different writers. I fhall endeavour to convey fome idea of the bufinefs, in as few words as I can.

Defcription of the fuppofed procefs of fnakes in fafcinating other animals.

The fnake, whatever its fpecies may be, lying at the bottom of the tree or bufh upon which the bird or fquirrel fits, fixes its eyes upon the animal which it defigns to fafcinate or enchant. No fooner is this done than the unhappy animal (I ufe, for the prefent, the language of thofe who differ from me in opinion, on this fubject) is unable to make its efcape. It now begins to utter a moft piteous cry, which is well known by thofe who hear it, and underftand the whole machinery of the bufinefs, to be the cry of a creature enchanted. If it is a fquirrel, it runs up the tree for a fhort diftance, comes down again, then runs up, and, laftly, comes lower down. "On that occafion," fays an honeft but rather credulous writer *, "it has been obferved, that the fquirrel always goes down more than it goes up. The fnake ftill continues at the root of the tree, with its eyes fixed on the fquirrel, with which its attention is fo entirely taken up, that a perfon accidentally approaching, may make a confiderable noife, without the fnake's fo much as turning about. The fquirrel as before mentioned comes always lower, and at laft leaps down to the fnake, whofe mouth is already wide open for its reception. The poor little animal then with a piteous cry runs into the fnake's jaws, and is fwallowed at once, if it be not too big; but if its fize will not allow it to be fwallowed at once, the fnake licks it feveral times with its tongue, and fmoothens it, and by that means makes it fit for fwallowing †."

Other accounts are not materially different.

It would be eafy to cite, from different authors, other accounts of the manner in which the enchantment is performed; or, more properly fpeaking, of the conduct, or behaviour, of the enchanting and enchanted animals. But between thefe accounts, there is hardly a fpecific difference. There is confiderable unity in all the relations that I have heard, or read.

* Profeffor Peter Kalm.

† Travels into North-America; containing its natural hiftory, and a circumftantial account of its plantations and agriculture in general, &c. &c. vol. i. p. 317 & 318. Alfo vol. ii. p. 207, 208, 209 & 210. Englifh Tranflation. London: 1770 & 1771.

However,

However, thofe who wifh to examine this part of the fubject more fully, will, at leaft, receive fome degree of entertainment from the perufal of the many authors who have believed and afferted, that ferpents poffefs a power of fafcinating other animals.

That the belief in the exiftence of this power fhould have been fo general among the uninformed part of a people, ought not to be wondered at. The human mind, unenlightened by fcience, or by confiderable reflection, is a foil rich in the weeds of fuperftition, and credulity. It is ever prone to believe in the wonderful, even when this belief, as is often the cafe, brings with it fears, and cares, and mifery. The bondage of the mind in fuperftitious credulity is great and heavy. Neither religion nor virtue can give it its freedom. This it obtains from fcience. How important, then, even in this point of view, is the enlargement of the mind by fcience! — Credulity in the vulgar is to be expected;

But it is, furely, a matter of fome aftonifhment, that this belief fhould have been admitted, in all the fulnefs of its extravagance, by men of learning, of obfervation, and of genius: by thofe who have the book of nature in their hands; that book which will, in fome future and fome happier age, eradicate many of the prejudices which disfigure, and which mock the dignity of human nature: by claffical fcholars, grown old in the difbelief of fimilar fables, heightened and embellifhed by the charms of poetry; and alfo by the infidel, who denies the authenticity of fcripture-miracles, few of which, even though they were not fhown to be truths, are more improbable than the imaginary fact which I am examining. — but it is ftrange that men of ability have alfo admitted thefe facts.

I have fought to difcover the original, or fource of this belief. I do not find any traces of it among the ancient writers of either Greece or Rome. I think, it is moft likely that no fuch traces can be found. Lucan, had ferpents been thought to poffefs a fafcinating faculty in his age, and in the country in which he lived, would, probably, have availed himfelf of its exiftence, in his beautiful account of the march of Cato's army through the Libyan-Defert*; and had fuch a notion prevailed in the earlier days of Lucretius, would we not find fome mention made of it in the poem *De Rerum Natura*, one of the fineft and moft varied productions of the human mind? Claffical fcholars may poffibly, however, difcover the dawn — This notion is not found in the writings of the ancients.

* Pharfalia, lib. IX.

of this notion in Greek and Roman authors, unread by me. On this fubject, I have not pufhed my inquiries as far as I wifhed to have done. It is not unlikely that I may examine the queftion more curioufly, at fome future period.

It is probable that in the mythology of Afia and of Africa, we fhall difcover fome traces of this notion, fo intimately connected with the fuperftitious credulity of a people, and even fo naturally arifing out of an imperfect view of the manners of ferpents.

It does not appear to have originated nor to be much, if at all, admitted among the Indians.

If we believe the Reverend Dr. Cotton Mather *, Mr. Dudley †, and other perfons, who had refided in North-America, we are to look for the beginning of this ridiculous notion among our Indians. How far, however, this is really the cafe may, I think, be doubted. It is certain that, at prefent, the opinion is by no means univerfal among the Indians. Several intelligent gentlemen, who are well acquainted with the manners, with the religious opinions, and with the innumerable fuperftitious prejudices of the Indians, have informed me, that they do not think thefe people believe in the notion in queftion. My friend Mr. John Heckewelder, of Bethlehem, writes to me, that he does not recollect to have heard the Indians fay that fnakes charm birds; though he has frequently heard them fpeak of the ingenuity of thefe reptiles in catching birds, fquirrels, &c. Mr. William Bartram fays, that he never underftood that the nations of Indians among whom he travelled had any idea of the fafcinating power of fnakes ‡. On the other hand, however, a Mohegan-Indian told me that the Indians are of opinion that the rattle-fnake can charm, or bewitch, fquirrels and birds, and that it does this with its rattle, which it fhakes, thereby inviting the animals to defcend from the trees, after which they are eafily caught. According to this Indian, his countrymen do not think that the fnake, in any manner, accomplifhes the bufinefs with its eyes. A Choktah-Indian affured me that the rattle-fnake does charm birds, &c. but he was honeft enough to confefs that he

* The Philofophical Tranfactions, abridged, vol. v. part ii. no. 339. p. 162.

† Ibid. vol. vi. part iii. no. 376. p. 45.

‡ MS. note, communicated to me by this ingenious gentleman.

did not know in what manner it does it. The interpreter, through whom I converfed with this Indian, faid that the fnake charms by means of its rattle.

The veneration, or regard, which has been paid to the rattle-fnake by certain North-American tribes feems, at firft fight, to favour the opinion, that thefe tribes attributed to this hideous reptile fome hidden power *, perhaps that of fafcinating animals. Mr. William Bartram informs me, that the fouthern Indians, with whom he is acquainted, feem to hold the rattle-fnake in a degree of veneration †. Mr. Heckewelder fays that, to his certain knowledge, this reptile was once held in particular efteem by the Delawares. He was feveral times prevented, by thefe Indians, from killing the rattle-fnake, being told that it was their grand-father, and, therefore, muft not be hurt. At other times, he was told, he muft not kill this fnake, becaufe the whole race of rattle-fnakes would grow angry, and give orders to bite every Indian that might come in their way ‡. But, of late, efpecially among thofe Indians who have had connection with the whites, thefe ridiculous notions have mouldered away, and our Indians, at prefent, kill their rattling "grandfather" with as little ceremony as the Efkemaux are faid to kill their parents in old-age.

Some North-American tribes had a veneration for the rattle-fnake;

It is obvious, from contemplating the manners and the hiftory of nations, that a part of their religions, and a large part of the fabrick of their fuperftitious notions, have arifen

which feems to have been part of the religious fyftem grounded on fear.

* Vis abdita. Lucretius.

† MS. note communicated to me.

‡ In my *Hiftorical and Philofophical Inquiry* (not yet publifhed), I have collected many facts which feem inconteftibly to prove, that the mythology, or fuperftitious religion, of the Americans is a fragment of that mythology whofe range in Afia, and in Africa, has been fo extenfive. Poffibly, the veneration, or regard, which was paid to different kinds of ferpents in America did not originate in this continent, but had its fource in Afia, from which portion of the globe (after a long and laborious attention to the fubject) I cannot doubt, that almoft all the nations of America are derived. It is unneceffary, in this place, to cite inftances of the religious veneration which was, and ftill is, paid to fome fpecies of ferpents, in various parts of the old-world. Thefe inftances muft be familiar to every perfon, who is acquainted with the hiftorians or with the poets of antiquity, and with the hiftory of the Gentoo-Indies.

out of fear. Perhaps, all mankind * admit the exiftence of two great beings, the one good and all-benevolent, the other bad and ftudious of evil. In our own continent, where, I believe, this notion was univerfal, certain tribes were affiduous in their adoration of the latter being, whilft the former, whom the light of reafon taught them to confider as the fource of life, and all their bleffings, was merely acknowledged and named, but unworfhipped and neglected †. The Delawares, and fome other nations who fpeak dialects of their language, believe that a turtle, of an enormous fize, inhabits the deep, and fupports upon his back this continent, or, as they call it, ifland. They fay it is in the power of this animal, by diving, to drown the world, as he has already done, in former ages. They, therefore, endeavour to conciliate his friendfhip and good-will. With this view, they make rattles of the turtle-fhell, into which they put fmall ftones, beans, or Indian-corn ‡, and play with this inftrument, at their dances. The turtle is greatly efteemed by them; and, in the fulnefs of a mixed zeal and fear, they even deign to call him *Mannitto*, or God; becaufe, they fay, he can live both upon the land and in the water §.

It is not likely that they venerated the fnake from a belief of its fafcination.

It feems very probable to me, that the veneration for the rattle-fnake had its birth in fear, and not in the belief that this reptile poffeffed the power of fafcinating animals. If, as fome writers have afferted, the Indians were in poffeffion of abfolute fpecifics for the bite of the rattle-fnake, I am of opinion that the veneration for this animal would not have exifted; or, at leaft, that it would not long have continued. But the Indians are often unable to prevent or to cure the effects of the active poifon of this ferpent, which not unfrequently deftroys them ‖.

I return

* I fpeak of mankind in the aggregate, and not of individuals among them.

† John De Laet, fpeaking of the Indians of New-York, has the following words: "Cæterum nullus ipfis religionis fenfus, nulla Dei veneratio: diabolum quidem colunt fed non tam folemniter neque certis ceremoniis, ut Africani faciunt," &c. Novus Orbis feu Defcriptionis Indæ Occidentalis Libri xviii. lib. iii. cap. xi. p. 75. Lugd. Batav. 1633.

‡ Maize.

§ MS. by Mr. John Heckewelder, *penes me*.

‖ Adair fays, he does "not remember to have feen or heard of an

I return to the more immediate path of my subject.

Among the Indians of South-America, I do not find any traces of the notion that serpents can fascinate other animals. Piso, the author of the *Natural and Medical History of the two Indies*, seems to have been studious to bring together the extraordinary things which have been related of the rattle-snake. But he says not a syllable concerning the fascinating faculty of this reptile *.

The South Americans do not possess this notion,

But whatever may have been the native country of the notion which I am considering, it would have been well had it been confined to savages. It is a tale which seems nicely adapted to the wit and society of rude and uncultivated nations. Unfortunately, the progress of error and credulity is extremely rapid. Their dominion is extensive. The belief in the fascinating faculty of serpents has spread through almost all the

but it has spread over all America and Europe.

Indian dying by the bite of a snake, when out at war, or hunting; although they are then often bitten by the most dangerous snakes." *The History of the American Indians*, &c. p. 235. London: 1775. It is certain, from the testimony of many persons, that the bite of the rattle-snake has often proved mortal to the Indians, and others, notwithstanding the boasted specifics of these people. Father Cajetan Cattaneo says, many Indians die with the bite of serpents. "But," observes the father, "it is said they commonly escape with life, when they can quickly apply the remedy which providence has prepared of certain herbs, especially the spikenard, which some parts of *Paraguay* produce in plenty. But when they are bit by the rattle-snake it is confidently assured that the case admits no cure." *The third letter of F. Cajetan Cattaneo.* See *A Relation of the missions of Paraguay, wrote originally in Italian, by Mr. Muratori.* English Translation. p. 260. London: 1759. Father Cattaneo is here speaking of the South-American rattle-snake, the poison of which, I have little doubt, is more deleterious than that of the same animal in our part of North-America. Still, however, I am confident, that this poison, even in the most fervid climates, is not always mortal.

* Gulielmi Pisonis medici Amstelædamensis de Indiæ utriusque re naturali et medica libri quatuordecim. Amstelædami: apud Elzevirios, 1658. Some of Piso's assertions concerning the rattle-snake are very extravagant. Such are the following: "Caudæ extremitate in anum hominis immissa, mortem infert confestim; venenum autem quod ore vel dentibus infundit, multo lentius vitam tollit." p. 275.

civilized

civilized parts of North-America. Nor is it confined to America. It has made its way into Europe, and has there taken poffeffion of the minds of fcholars, of naturalifts, and of philofophers.

It is faid that Dr. Johnfon believed it. But, he was credulous, timid, and melancholy.

I think, I have fomewhere either heard or read that the tale was credited by the late Dr. Samuel Johnfon. If I am miftaken, I hope the admirers of this great man, fhould any of them read my memoir, will pardon me. It is certain, notwithftanding the vaft ftrength and the rich fertility of Johnfon's mind, that he was credulous and timid. Did this union of credulity and timidity arife out of that unhappy melancholy (" thofe cafual eclipfes which darken learning",) that often overclouded the brightnefs of the mind*? We are told that the Hercules of Englifh literature believed in ghofts, and in the fecond-fight. The man who would thus fuffer his mind to be eftranged from probability, and entangled in difficulties, would, perhaps, find it eafy to bend to the belief, that ferpents have the faculty of fafcinating other animals.

Linnæus admits it.

Although I profefs myfelf to be a warm admirer of Linnæus, and although, at a very early period of my life, I enlifted myfelf under the banner of his fchool, I fhall not, neverthelefs, attempt to conceal, that this great man gave credit to the tale of the fafcination of birds and other animals by ferpents. In his *Syftema Naturæ* (that immortal work), under the article Crotalus horridus, or the rattle-fnake, he has the following words: " *Aves Sciurofque ex arboribus in fauces revocat.*"†. In another work, he fpeaks as follows. " Whoever is wounded by the Hooded Serpent *(Coluber Naja)* expires in a few minutes; nor can he efcape with life who is bitten by the rattle-fnake, *(Crotalus horridus)* in any part near a great vein. But the merciful God has diftinguifhed thefe pefts by peculiar

* Or, did his melancholy grow out of his credulity and fear?

† See volume firft, p. 372. Vienna edition of 1767. Profeffor Gmelin, in his edition of the *Syftema Naturæ*, when fpeaking of the rattle-fnake, has the following words, viz. "*aves fciurique ex arboribus non raro in fauces inhiantis apertas incidunt,*" tom. i. pars iii. p. 1080. The fame laborious author fpeaking of our grey-fquirrel (Sciurus cinereus) fays, " a crotalo comeditur," tom. i. p. 147. This is true: but he might have faid the fame when fpeaking of the ftriped-dormoufe, or ground-fquirrel (Sciurus ftriatus), of our rabbit (Lepus americanus), and many other animals.

fign,

figns, and has created them moft inveterate enemies; for as he has appointed cats to deftroy mice, fo has he provided the Ichneumon *(Viverra Ichneumon)* againft the former ferpent, and the Hog to perfecute the latter. He has moreover given the *Crotalus* a very flow motion, and has annexed a kind of rattle to its tale, by the motion of which it gives notice of its approach: but, left this flownefs fhould be too great a dif- advantage to the animal itfelf, he has favoured it with a certain power of fafcinating fquirrels from high trees, and birds from the air into its throat, in the fame manner as flies are precipitated into the jaws of the lazy toad."* His account.

Linnæus was, certainly, extremely credulous, though I do not find that any of his profeffed biographers have taken notice of this feature of his mind. But the proofs of my obfervation are numerous: they are to be found in almoft every effay that he has written. His credulity with refpect to the powers of medicines is, perhaps, peculiarly ftriking †. How far this credulity, in a mind otherwife truly great (a mind which with refpect to the arrangement of natural bodies has never been equalled), is to be fought for in the general character of the country which gave Linnæus birth, I fhall not paufe to inquire. Yet in an inveftigation of this kind, where the opinion of the Swedifh Pliny is neceffarily mentioned, it might be curious to look to the fources of his credulity. The ftudy of nature, as it refpects this globe, is, perhaps, of all the fciences, the moft unfavourable to fuperftition, or credulity. But the greateft of naturalifts was one of the moft credulous of philofophers. It is certain that Linnæus was credulous.

It is proper, however, to obferve, in this place, that Linnæus by no means afferts, that he himfelf had ever been a He never was himfelf witnefs to the fact.

* See Reflections on the Study of Nature, tranflated from the Latin of Linnæus. p. 33 & 34. Dublin edition, 1786. Dr. I. E. Smith, the ingenious tranflator of this differtation, in a note to the above paffage, has the following words. "This opinion of the fafcinating power of the Toad has been refuted, and the appearance which gave rife to it fully accounted for, by Mr. Pennant, in his Britifh Zoology. Probably the ftory of the rattle-fnake's having a fimilar power might be found equally falfe, if enquired into with the fame degree of accuracy." p. 34.

† See his Materia Medica, liber. i. de Plantis, &c. Amftelædami: 1749.

witnefs

witneſs to the faſcinating power of any of the ſerpent-tribe. He ſeems to have received the tale from ſome of his many pupils, whom he animated with the love of natural hiſtory. It is probable that Kalm, whom Linnæus quotes upon various occaſions, and whom he could not but eſteem, principally contributed to fix his illuſtrious maſter's credulity in this reſpect: for, in different parts of his *Travels*, this induſtrious author has given his decided aſſent to the tale; and he informs us, that he has treated of the ſame ſubject, more fully, in a treatiſe which is printed in the *Memoirs of the Royal Swediſh Academy of Sciences*, for the year 1753 *.

Nor was Kalm; though he ſupports it,

Kalm is candid enough to tell us, that he never ſaw an inſtance of the faſcinating power of the ſerpent-kind. "However," ſays he, "I have a liſt of more than twenty perſons, among which are ſome of the moſt creditable people, who have all unanimouſly, though living far diſtant from each other, aſſerted the ſame thing †." He then goes on to tell us a long ſtory, ſimilar to that which I have related, in the beginning of this memoir, and which, therefore, it is not neceſſary to repeat, in this place.

and ſpeculates upon it.

Our author is not content to make mere mention of the fact; he undertakes to ſpeculate upon it. And here, although a talent for ingenious reaſoning is, certainly, not the moſt ſtriking feature that is diſplayed in the *Travels* of Kalm, he acquits himſelf, for ſome time, very judiciouſly; but ſpoils all he has ſaid, by concluding, that the bird or ſquirrel "are only *enchanted*, whilſt the ſnake has its eyes fixed on them.‡" He allows that "this looks odd and unaccountable, though," ſays he, "many of the worthieſt and moſt reputable people have related it, and though it is ſo univerſally believed here," that is in New-Jerſey, &c. "that to doubt it would be to expoſe one's ſelf to general laughter §."

Several American writers have adopted the notion, that ſnakes are endued with a faſcinating faculty. Fearful that their authority may extend the empire of this error, I have been the more anxious to offer my ſentiments on the ſubject to the ſociety ‖.

It

* Travels into North-America, &c. vol. i. p. 318 & 319.

† Ibid. vol. ii. p. 207 & 208.

‡ Travels into North-America, &c. vol. ii. p. 210.

§ Ibid.

‖ Speaking of the rattle-ſnake, my worthy friend Mr. William Bartram

Reſpectable European naturaliſts doubt it.

It has given me pleaſure to find, that the enchanting faculty of the rattle-ſnake is doubted by ſome very reſpectable European naturaliſts. "It is difficult," ſays my excellent friend Mr. Pennant, "to ſpeak of its faſcinating powers: authors * of credit deſcribe the effects. Birds have been ſeen to drop into its mouth, ſquirrels deſcend from their trees, and leverets run into its jaws. Terror and amazement ſeem to lay hold on theſe little animals: they make violent efforts to get away, ſtill keeping their eyes fixed on thoſe of the ſnake; at length, wearied with their movements, and frightened out of all capacity of knowing the courſe they ought to take, become at length the prey of the expecting devourer, probably in their laſt convulſive motion †."

The ſubject treated by la Cépède.

My friend Mr. de la Cépède, one of the moſt eloquent naturaliſts of the age, has devoted a good deal of attention to the ſubject, in his *Hiſtoire Naturelle des Serpens*, a work of extenſive and ſuperior merit. I regret, however, that this ingenious author was not in poſſeſſion of a few facts, well known in this country, which could not have failed to conduct a mind, like his, ſtrengthened by the enlarged contemplation of the objects of nature, to the fulneſs and certainty of truth. As it is, however, Mr. de la Cépède deſerves our thanks for reviving, and giving a new turn to, the ſpeculations of naturaliſts on this ſubject.

Bartram ſays: "They are ſuppoſed to have the power of faſcination in an eminent degree, ſo as to inthral their prey. It is generally believed that they charm birds, rabbits, ſquirrels, and other animals, and by ſtedfaſtly looking at them, poſſeſs them with infatuation; be the cauſe what it may, the miſerable creatures undoubtedly ſtrive by every poſſible means to eſcape, but alas! their endeavours are in vain, they at laſt loſe the power of reſiſtance, and flutter or move ſlowly, but reluctantly towards the yawning jaws of their devourers, and creep into their mouths, or lay down and ſuffer themſelves to be taken and ſwallowed." Travels through North and South Cardinia, Georgia, Eaſt and Weſt Florida, &c. p. 267. Philadelphia: 1791.

* "Lawſon—Cateſby—Ph. Tr. abridg. ix. 56, &c. vii. 410.—Brickel's Hiſt. Carolina, 144.—Beverley Virginia, 260.—Colden, i. 12. Dr. Brickel is an author of no credit. His *Hiſtory of North-Carolina*, here quoted is one of the moſt daring and ſcandalous inſtances of plagiariſm I am acquainted with.

† Arctic Zoology, vol. ii, p. 338. London: 1792.

I beg

I beg leave, in this place, to quote that part of Mr. de la Cépède's work which relates to the queſtion of my memoir.

His account. Suppoſition of infection in the breath.

Speaking of the boiquira, or rattle-ſnake, my ingenious friend has the following words: "His infectious breath, which ſometimes agitates the little animals he is about to ſeize, may alſo prevent their eſcape. The Indians relate, that a rattle-ſnake is often ſeen, curled round a tree, darting terrible glances at a ſquirrel, which after expreſſing its fear by its cries and its tremour, falls at the foot of the tree, where it is devoured. Mr. Voſmaer (at the Hague), who has made ſeveral experiments on the bite of a rattle-ſnake, which he had alive, ſays that the birds and mice, which were thrown into the cage, would immediately endeavour to ſquat in a corner, and that ſoon after, as if ſeized with deadly anguiſh, they would run towards their enemy, who continually ſhook his rattles: but this effect of a mephitick and fetid breadth has been ſo much exaggerated, and miſrepreſented, that it becomes miraculous.

Faſcinating property ſuppoſed to ariſe from this cauſe.

"It has been ſaid," continues our author, "that the rattle-ſnake had a faculty of enchanting, as it were, the animal he intended to devour; that by the power of his glance, he could oblige the victims to approach by ſmall degrees, and finally to fall into his mouth; that even man could not reſiſt the magic force of his ſparkling eyes; and that under violent agitations he would expoſe himſelf to the envenomed tooth of the ſerpent, inſtead of endeavouring to eſcape. If the rattle-ſnake had been more generally known, and if his natural hiſtory had engaged more attention, other circumſtances, ſtill more extraordinary, would have been added to theſe miraculous feats; and how many fables would not have been ſubſtituted to the ſimple effect of a peſtilential breath, which, however, has by no means been either ſo frequent or ſo fatal as ſome naturaliſts have believed.

Or that the animal may have been already bitten.

"We may preſume, with Kalm, that, for the moſt part, when a bird, a ſquirrel, or any other animal, has been ſeen precipitating itſelf from the top of a tree into the jaws of a rattle-ſnake, it had been already bitten *; that after eſcaping, it

* I do not find that Kalm has adopted this ſyſtem of explanation, in his *Travels*. On the contrary, in this work, he gives ſome judicious reaſons for rejecting this mode of explanation. *Travels*, &c.

manifested, by its cries and its agitation, the violent action of the poison left in its blood, and diffused through its circulation, by the envenomed inoculation of the reptile's tooth; that, its strength gradually decaying, it would fly or leap from branch to branch, till finally exhausted it would fall before the serpent, who with inflamed eyes, and eager looks, would watch attentively every motion, and then dart on his prey, when it retained but a small portion of life. Several observations related by travellers, and particularly a fact mentioned by Kalm, appeared to confirm this *."

Examination of these doctrines.

From this long quotation, it appears that Mr. de la Cépède adopts two modes, or circumstances, for explaining the miraculous power, which has been attributed to these serpents. The explanation is, undoubtedly, in both cases, ingenious, and entitled to notice. I shall examine the question with that attention which it deserves.

In the first place, my learned friend supposes, that the rattle-snake's infectious breath †, by agitating the little animals which it means to devour, may prevent their escape.

Observation to show that snakes do not emit any offensive odour,—

I do not altogether understand this expression of an infectious breath. I do not think that we are in possession of any facts by which it can be proved, that the breath of the rattle-snake is, in general, more infectious, or pestiferous, than that of many other animals, whether of the same or of a different family. I know, indeed, that in some of the larger species

&c. vol. ii. p. 209 & 210. His memoir, in the *Swedish Transactions*, I have not seen. Sir Hans Sloane, a long time since, conjectured, that the whole mystery of the fascinating faculty of the rattle-snake is this, viz. "that when such animals as are the proper prey of these snakes, as small quadrupeds, birds, &c. are surprised by them, they bite them, and the poison allows them time to run a small way; or perhaps a bird to fly up into the next tree, where the snakes watch them, with great earnestness, till they fall down, or are perfectly dead, when having licked them over with their spawl or spittle, they swallow them down." *Philosophical Transactions*, vol. xxxviii. no. 403. M. de la Cépède does not appear to have seen Sloane's paper.

* Histoire Naturelle des Serpens, p. 409, 410 & 411. a Paris: 1789.

† His words are, "son haleine empestée, qui trouble quelquefois les petits animaux dont il veut se saisir, peut aussi empêcher qu'ils ne lui échappent." p. 409.

of

of ſerpents, inhabiting South-America, and other countries, there is evolved in the ſtomach, during the long and tedious proceſs of digeſtion in theſe animals, a vapour, or a gas, whoſe odour is intenſely fetid. I have not, however, found that this is the caſe with the rattle-ſnake, and other North-American ſerpents, that I have examined. But my own obſervations on this head have not been very minute. I have made inquiry of ſome perſons (whoſe prejudices againſt the ſerpent-tribe are not ſo powerful as my own), who are not afraid to put the heads and necks of the black-ſnake, and other ſerpents that are deſtitute of venomous fangs, into their mouths, and have been informed, that they never perceived any diſagreeable ſmell to proceed from the breath of theſe animals. I have been preſent at the opening of a box which contained a number of living ſerpents; and although the box had been ſo cloſe as to admit but a very ſmall quantity of freſh air, although the obſervation was made in a ſmall warm room, I did not perceive any peculiarly diſagreeable effluvium to ariſe from the bodies of theſe animals. I am, moreover, informed by a member of this ſociety *, who, has, for a conſiderable time, had a rattle-ſnake under his immediate care, that he has not obſerved that any diſagreeable vapour proceeds from this reptile. On the other hand, however, it is aſſerted by ſome creditable perſons of my acquaintance, that a moſt offenſive odour, ſimilar to that of fleſh, in the laſt ſtage of putrefaction, is continually emanating from every part of the rattle-ſnake, and ſome other ſpecies of ſerpents. This odour extends, under certain circumſtances, to a conſiderable diſtance from the body of the animal. Mr. William Bartram aſſures me, that he has obſerved "horſes to be ſenſible of, and greatly agitated by, it at the diſtance of forty of fifty yards from the ſnake. They ſhowed," he ſays, "their abhorrence, by ſnorting, winnowing, and ſtarting from the road, endeavouring to throw their riders, in order to make their eſcape †." This fact related by a man of rigid veracity, is extremely curious; and, in an eſpecial manner, deſerves the attention of thoſe writers, who, like Mr. de la Cépède, imagine that this fetid emanation from ſerpents is capable of affecting birds,

and others to ſhow that they do.

* Mr. Charles Wilſon Peale.

† MS. note, communicated to me.

at ſmall diſtances, with a kind of aſphyxy *. It even gives *ſome* colour of probability to the ſtory related by Metrodorus, and preſerved in the *Natural Hiſtory* of Pliny †.

*(To be continued.)*

## VIII.

*Extract of a Letter from Brugnatelli on the Pruſſiate of Potaſh, and on the Preparation of a fulminating Muriate of Silver.*‡

If alcohol be poured into a ſolution of pruſſiate of potaſh (oxipruſſiate of potaſh,) the liquor becomes turbid, and a precipitate is depoſited in the form of ſmall and very brilliant lamellæ, reſembling the ſublimed boracic acid (oxiboracic.) Collecting this precipitate on a filter, and drying it, the ſcales adhere to one another, and form a ſtratum of a ſhining yellowiſh white, like that of mother-of-pearl. This maſs eaſily ſeparates from the filter; is ſoluble in ſulphuric acid (oxiſulphuric), and this ſolution takes a blue colour on the addition of water. Expoſed to the fire in a crucible, it ſwells, burns, and leaves a coally reſiduum, containing a great deal of iron, attractable by the magnet and potaſh. This coally reſiduum, treated with acids, gave out a ſtrong ſmell of ſulphurated hidrogen gas *(gas phlogogène ſulfuré.)*

Alcohol occaſions a precipitate in ſolution of pruſſiate of potaſh, which is ſmall ſcales, adhering to each other, of a ſhining, yellowiſh white, ſoluble in ſulphuric acid; and this ſolution is turned blue by water.

Its coally reſiduum contains a great deal of magnetic iron, and potaſh; and with acids ſmells ſtrongly of ſulphurated hidrogen gas.

I have applied the proceſs I mentioned to you in my laſt to a great number of other metallic oxides (thermoxides), in hope of rendering them fulminating; but I ſucceeded with none, except that of ſilver.

Take a hundred grains of lunar cauſtic (fuſed oxiſeptonate of ſilver) in powder, put them into a beer glaſs, and pour on them firſt an ounce of alcohol, and then as much concentrated nitrous acid *(oxiſeptoneaux.)* The mixture grows hot, enters into a ſtate of ebullition, and an ether is viſibly formed, that changes into a gas. By degrees the liquor becomes milky and opake, and is filled with ſmall and very white clouds. When all the gray powder of the lunar cauſtic has taken this form, and the liquor has acquired a conſiſtency, diſtilled water muſt be

On 100 grains of nitrate of ſilver powdered was formed firſt, alcohol 1 ounce, then nitrous acid 1 ounce.

As ſoon as the precipitate was completely formed, water was poured on, and the precipitate dried in a filter.

* Hiſtoire Naturelle des Serpens, p. 355.

† Lib. xxviii. cap. 14.

‡ Van Mons's Journal, IV. 235.

be added immediately, to fuspend the ebullition, and prevent the matter from being rediffolved, and becoming a mere folution of filver. The white precipitate is then to be collected on a paper filter, and dried.

Its weight more than 50 grains. It detonates more ftrongly than fulminating mercury, being touched with a glafs tube wetted with fulphuric acid,

This precipitate is fulminating filver, and amounts to more than half the weight of the lunar cauftic employed. The detonating power of this preparation greatly exceeds that of fulminating mercury, prepared in Howard's or my way, even in much fmaller quantity. It detonates in a tremendous manner, on being fcarcely touched with a tube of glafs, the extremity of which has been dipped in concentrated fulphuric acid *(oxifulfurique)*, even in that of the fhops. One grain of this fulminating filver, placed on a lighted coal, gave fuch a loud report, that it deafened the ears of the perfons prefent. The fame thing happens on placing a little of the fame preparation on an electric pile, with the interpofition of a bit of paper, and drawing a fpark from its centre by means of a flip of metal. The paper will be either pierced or torn.

placed on a lighted coal, or having a fpark drawn through it from the electric pile.

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## IX.

### *Of the Application of Platina or Porcelain. By* KLAPROTH.†

Platina never yet applied on porcelain.

PLATINA, as far as I know, has never yet been employed in encauftic painting. Accordingly I thought it not amifs, to make fome experiments on the fubject, the refult of which did not deceive my expectations.

Gold and filver only have been ufed in this way in the metallic ftate: and gold anfwers completely, but filver does not cover the ground fo well, and is liable to tarnifh.

Gold and filver are the only metals, that have hitherto been employed in the metallic form in painting or ornamenting porcelain, glafs, or enamel. Gold anfwers this purpofe fo completely, that it leaves nothing to be defired: but filver is far from affording equal fatisfaction. As it is of lefs denfity than gold, it cannot be ufed fo thin, and does not cover the porcelain or other fubftance fo well. Another reafon why it is lefs applicable to the purpofe of painting on porcelain is its property of having its metallic luftre tarnifhed by fulphurous emanations, all kinds of which blacken it. This unfavourable circumftance prevents filver from being employed in fine enamel painting, and confines the ufe of the metallic fubftances in that only to gold.

* Abridged from Scherer's *Journal der Chimie*, 1802. No. 4. p. 413.

Platina in its qualities ranks with gold, and by its whitenefs fupplies the place of filver, without labouring under its defects. It not only covers the ground well by its denfity, which even furpaffes that of gold; but like this it refifts all the alternations of the atmofphere, and is not any way tarnifhed by fulphurous effluvia.

Platina imitates the whitenefs of filver, without having its defects.

The procefs for applying it is very fimple. Platina is diffolved in nitro-muriatic acid, and precipitated by a folution of muriate of ammonia. The red cryftalline precipitate formed is to be dried, reduced to a fine powder, and made flightly red hot in a glafs retort. The muriate of ammonia, which had precipitated in combination with the platina, fublimes; and the metal remains at the bottom of the retort, in the form of a light, gray powder. This powder being mixed with a fmall proportion of flux, as is done with gold, and ground with oil of fpike, is to be applied to the porcelain, put into the furnace, and afterwards burnifhed.

Procefs of applying it.

Platina applied on porcelain in this manner, is of a filver white, flightly tending to the gray of fteel. By alloying this metal in different proportions with gold, different fhades of this colour are obtained. Platina admits a confiderable quantity of gold, before its colour undergoes any perceptible change to yellow. For example, if one part of platina be alloyed with four parts of gold, the prefence of the latter cannot be perceived, and the colour fcarcely differs from that of pure platina. The colour of the gold does not predominate, unlefs it be in the proportion of eight to one.

It refembles filver with a flight fteel tinge. Gold in different proportions varies its fhade; but does not render it perceptibly yellow, unlefs 4 parts of gold be added to 1 of platina; and the gold colour does not predominate, unlefs it be in the proportion of 8 to 1.

The alloys of platina with filver give only a dull metal.

Platina and filver give a dull alloy.

Befide this method of applying platina to porcelain, it may be laid on in the ftate of folution. In this way its colour, luftre, and appearance, are very different. If the nitro-muriatic folution of platina be evaporated to a certain confiftence, and laid on the porcelain feveral times, the metal penetrates into its fubftance, which, after it comes out of the furnace, exhibits a metallic mirror of the colour and brilliancy of polifhed fteel.

Platina may likewife be applied in folution; and this, if feveral coats be laid on, penetrates the fubftance, and produces a mirror refembling polifhed fteel.

X. *Abftract*

## X.

*Abstract of a Memoir on Galvanism, sent to the National Institute by Mr.* RITTER, *of Jena.*†

Erman discovered, that an imperfect conductor would take the electricity from an insulated pile, and become charged like it, one end +, the other —.

TO form a just idea of these researches, it is necessary to recal to mind a discovery made by Mr. Erman of Berlin, about two years ago, and since repeated by Volta before the galvanic committee of the Institute.

If an electric pile, the superior pole of which is positive, and the inferior negative, be insulated, and a communication is made between these two poles, by means of an imperfect conductor, as for instance, a slip of paper wetted with pure water would be for such small quantities of electricity; each moiety of this slip would take the electricity of the pole with which it communicated; the upper part would be positive, the lower negative.

If this be removed by a glass rod, its equilibrium will not be restored at once;

Now let us conceive this imperfect conductor to be removed by an insulating substance, as a glass rod: the equilibrium between the two extremities will not be restored instantaneously, but they will remain positive and negative for some time, as when they communicated with the two poles of the pile.

but gradually.

This difference will gradually diminish in proportion as the opposite electricities recombine, and their action, being neutralized, will soon become altogether imperceptible.

For this Mr. Ritter substitutes a pile of copper and wet pasteboard in alternate pieces.

The fundamental experiment of Mr. Ritter is precisely reducible to this; he merely substitutes for the slip of paper, a pile composed of disks of copper and wet pasteboard. This pile, incapable itself of setting electricity in motion, at least if we suppose all its component parts homogeneal, is charged by communication with the pile, like the slip of wet paper above-mentioned.

Weak electricity, like light, does not pass freely from one surface to another. Hence Mr. Ritter's pile remains charged for some time;

But there is an essential difference in the results. It appears that electricity, when weak, experiences, like light, a kind of difficulty in passing from one surface to another: at least this seems to follow from the experiments of Mr. Ritter, as he himself observes. The electricity introduced into the pile made with one metal alone, experiences therefore some resistance in passing from the metal to the pasteboard; and this obstacle in-

* Bulletin des Sciences, No, 79, p. 145. October, 1803.

creases,

creafes, in proportion as the alternations are more numerous. Accordingly, this pile, when once charged, muft lofe its electricity very flowly, if there be no communication between its poles.

But if a communication between its two poles be eftablifhed, by means of a good conductor, the current of the two electricities and their combination, taking place fwiftly, will produce a difcharge, which will act, as in the Leyden phial, by an inftantaneous fhock. To this effect a new ftate of equilibrium will fucceed, in which the intenfities of the different plates will be diminifhed in proportion to the quantity of electricity that is inftantaneoufly neutralized. Thefe difcharges muft therefore be reiterated; but growing feebler each time, as the contacts are repeated: but they will foon ceafe to be perceptible, in confequence of the general equilibrium they tend to reftore between all the parts of the apparatus; in a word, the action of this pile depends on its becoming alternately a better and worfe conductor, according as the two extremities are made to communicate with each other or not.

unlefs a communication be made between its poles, when it will give a fhock, like the Leyden phial.

Thus the intenfities of the plates will be diminifhed; till by repeated difcharges, each weaker and weaker, the equilibrium is reftored.

Its action explained.

As to the mode in which the electricity difpofes itfelf, it muft be fuch, that the repulfive power, or intenfity of each plate, combined with the refiftance of the furfaces, muft counterbalance the united actions of all the reft. In confequence, if the plates of metal be an uneven number, and the whole apparatus infulated, the intenfities will diminifh uniformly from the two extremities, where they will be equal and contrary, as in the primitive pile, to the centre, where they will be null: but if the bafe of the pile have a communication with the ground, the intenfities will increafe throughout the whole of the pile, from the bottom, where they will be null, to the top, where they will equal that of the primitive pile.

The electricity is greateft at the extremities, nothing in the centre, if infulated; or greateft at the top, and nothing at the bottom, if it communicates with the ground.

The apparatus juft defcribed, which Mr. Ritter calls a fecondary pile, produces fhocks, the decompofition of water, and all the other phyfiological or chemical effects obtained from the common pile, but with lefs intenfity. By varying the number and order of the difks of pafteboard and copper, Mr. Ritter obtained feveral interefting refults. Thus he obferved, that of all the modes, in which a certain number of heterogeneous conductors could be difpofed, the arrangement that has leaft alternation is moft favourable to the propagation of electricity. For inftance, if a pile be conftructed of fixty-four plates

This fecondary pile produces all the effects of the common pile, but weaker.

The fewer the alternations, the more fpeedily the electricity flows off,

plates of copper, and fixty-four pieces of wet pafteboard, difpofed in three parcels, all the pafteboards being placed together in the middle, and thirty-two plates of the metal at each end, this pile will conduct the electricity from Volta's very freely, and confequently charge itfelf very little. If the feries of wet conductors be interrupted by a fingle plate of copper placed in the midft of them, the conducting power will diminifh; more frequent interruptions will enfeeble it ftill more; and thus by increafing the number of interruptions we arrive at a feries, in which the conducting power will be fcarcely perceptible. Thefe phenomena led Mr. Ritter to the knowledge of the refiftance, which a weak electricity experiences in paffing from one furface to another: and this refiftance takes place only in a weak ftate, for by a fingular property, an electricity ftrong enough to overcome it opens itfelf a free paffage, and flows off completely.

and the contrary.

Though weak electricity experiences refiftance in paffing from one furface to another; electricity ftrong enough to overcome this paffes off at once.

We have juft feen, that the conducting power of an apparatus might be varied at pleafure, by altering its arrangement. It was natural to fuppofe, that thefe modifications would influence the chemical and phyfiological effects of the fecondary pile; and Mr. Ritter propofed to himfelf, to afcertain the differences of this influence.

As the arrangement affected the conducting power, it might be fuppofed it would influence the effects.

Accordingly he fought how to divide a given number of wet and folid conductors fo as to form a fecondary pile, that fhould receive the greateft poffible charge from a given electrical pile. Purfuing this inquiry, he difcovered two different arrangements, one of which gave a maximum of chemical effect, the other of phyfiological. The firft confifts in a fmall number of alternations. In this cafe the conducting power is very great, the flow of the fluid continual, and the phyfiological effect but weak. The fecond, on the contrary, confifts in more numerous alternations; in which the conducting power is much lefs, and takes place only at intervals, in momentary difcharges, when the refiftance of the furfaces is overcome. In this the electricity efcapes as it were by jirks, and the chemical effect refulting from it is fcarcely perceptible. Thefe differences appear to us to indicate, that the chemical effects depend particularly on a brifk continuous current of the fluid, while the phyfiological require fucceffive fudden difcharges, entering the organs as if by fhocks.

And it appeared, that the fewer the alternations, the greater the chemical effect; and on the contrary, the more numerous the alternations, the greater the phyfiological effect.

The former therefore is produced by a brifk continued flow of the electricity; the latter by repeated fhocks.

From

Ritter's pile well adapted to ſeparate the two actions.

From theſe principles we ſee, why the apparatus of Mr. Ritter is better adapted to ſeparate theſe two kinds of action than any other. In the common pile the intenſity increaſes with the number of pairs of metal, and balances the reſiſtance ariſing from theſe alternations; while in the ſecondary pile the intenſity of the two extremities can never ſurpaſs that of the primitive pile, and the reſiſtance produced by the alternations is entirely employed in modifying the current of a given quantity of electricity.

But Volta's pile could not charge Ritter's, if its electricity were not very weak.

In fine, if Volta's pile can thus change the ſecondary pile of Ritter, this property is owing to its intenſity being extremely weak, and as we may ſay imperceptible. A more powerful electricity, ſuch for example as that of the common electrical machine, would paſs through the apparatus completely and not produce the ſame effects.

Theſe deductions not advanced as certainties.

Though theſe deductions appear to us very natural, we offer them only with extreme reſerve, and becauſe we think they agree very well with the facts obſerved by Mr. Ritter.

Cauſes of difference in the ſecondary pile.

The differences that exiſt in the chemical action of common piles, in proportion to the ſize of the plates, occur likewiſe in the ſecondary piles. The arrangement of the pieces of paſteboard, their thickneſs, the nature of the ſolution with which they are wetted, and the order in which they are interpoſed, as well as various other trifling circumſtances, modify the effects in a thouſand ways, which it would be both uſeful and curious to inveſtigate.

It would be uſeful and curious to inveſtigate its effects.

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## XI.

*On Spectacles; particularly the Periſcopic. In a Letter from Mr.* E. Walker.

### To Mr. NICHOLSON.

*Lynn, March* 19, 1804.

SIR,

Spectacles have not been improved as much as other optical inſtruments.

ALTHOUGH the improvements which have been made in optics within the laſt century, have been ſuch as even to exceed the expectations of a Newton, yet our ſpectacles, the moſt uſeful of all optical inſtruments, have remained for ages in the ſame ſtate of imperfection. Many alterations, it is true,

have taken place in their form, but without producing any better optical effect; and this may occasion a suspicion, that the properties of the glasses made use of, have not been clearly understood.

General position that the rays ought to fall perpendicularly on the lens.

Emerson observes, that "when glasses are put into frames for spectacles, these frames ought not to be straight, so as both glasses may lie in the same plane; but they ought to be so bent in the middle, that the axis of both glasses may be directed to one point, at such a distance as you generally look with spectacles. By this means the rays will fall perpendicularly upon both glasses, and make the object appear distinct. But if they fall obliquely upon the glasses, it will cause a confused appearance in the object *." Martin also bent the frames of his visual glasses, for the same purpose as above described. And it appears from Dr. Wollaston's description of his *Periscopic Spectacles*, that they are also constructed on a principle to avoid oblique rays †.

Assertion: that the perpendicular rays are least adapted to distinct vision.

It is very singular, that this mode of constructing spectacles should have been recommended by so many writers on optics, when it is easily proved, that the rays of light which fall perpendicularly upon the centre of a lens, enter the eye placed near it, in a more confused state than those rays which fall upon a lens obliquely.

General obs. of Dr. Smith.

Dr. Smith, in his complete system of optics, gives us this general observation on vision. "That the apparent distinctness and confusion of an object depends upon the mutual inclination of the rays to each other in any one pencil when they fall upon the eye." ‡

Illustration; by which it is maintained that a pencil of rays passing near the axis of a lens will afford vision less distinct

This observation being admitted, let L L, *Fig.* 1. represent a double convex lens, *a o*, a ray of light passing perpendicularly through its centre; *c n*, and *d n*, two other rays nearly parallel to the former. The ray *a o* suffers no refraction; but the ray *c n* by passing through the lens, will be refracted towards the right hand, and the ray *d n*, by refraction, will be turned towards the left; consequently these rays have not a mutual inclination to each other when they fall upon the eye placed near the lens, as at *x*.

* Emerson's Optics, page 167.

† Philosophical Journal, V. VII. p. 143.

‡ Elementary parts of Smith's Optics, page 76.

4 But

Fig 1.

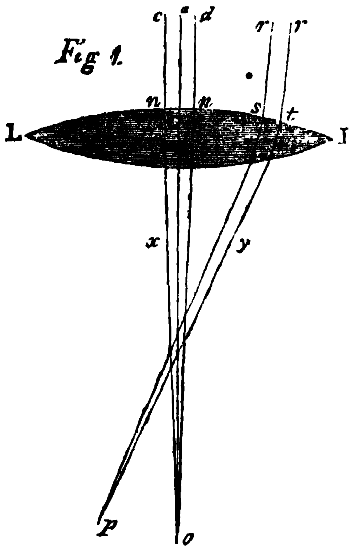


Fig 2.


But, let $rs$, $rt$, repreſent two other rays falling upon the lens in an oblique direction; theſe rays after paſſing through the lens, will both be refracted towards the left hand, and will therefore have "a mutual inclination" to each other when they fall upon the eye at $y$, turned to view an object in the direction $ry$; hence an object viewed in that direction, will be ſeen more diſtinctly, than if it were viewed through the centre of the lens.

—than a pencil paſſing farther from the center.

The ſame thing may be proved experimentally, thus; let a perſon when reading with ſpectacles that are proper for his ſight, look attentively for a moment, at a ſingle word through the centres of his glaſſes. Then let him turn his head, either to the right or to the left, (without moving the book) ſo that the ſame word may be ſeen by oblique rays, and it will appear much more diſtinctly than before.

Experiment offered in ſupport of this doctrine.

From what has been advanced it appears, that when glaſſes are put into frames for ſpectacles, thoſe glaſſes ſhould ſtand in an oblique direction, that the rays of light iſſuing from an ob-

Inference: That ſpectacle glaſſes ought to be fixed obliquely,

ject,

ject, may not fall perpendicularly upon them. Bending the frames will not effect this purpose, for the glasses must be parallel, as represents in *Fig.* 2. where A B represents the frame, *c c* and *d d*, the two glasses.

I have a pair of spectacles constructed in this manner, but so contrived, that the glasses may be set to any angle, to show the different effects of oblique and perpendicular rays.

The theory of Dr. Wollaston's patent spectacles, may be thus examined.

and that the meniscus is not superior to the double convex.

Let the lens L L be taken away from *Fig.* 1. and a meniscus of the same diameter and focal distance be drawn in its place. Then the rays *r s*, *r t*, after passing through this lens will meet at *(p)* as before, because this lens is of the same focal distance as the double convex, *by Hyp.*

Consequently these rays will fall upon the eye at *y*, in the same state of convergency as before. Whence a lens of the meniscus form, possesses no properties, *in theory*, superior to the double convex.

E. WALKER.

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## XII.

*Communication respecting the Chay Root, a Species of Madder used for dying durable red Colours in the East Indies. By* JOHN STOCKWELL, *Esq.* *

Introductory letter to Mr. Taylor.

MR. STOCKWELL presents his compliments to Mr. Taylor, with a sample of an article called Chaya Root, for the inspection of the Society for the Encouragement of Arts, &c. It is in much use on the Coromandel coast, for dying. He accompanies it with a short extract, which will afford some information concerning it.

If Mr. Taylor should cause any experiments to be made upon it, Mr. Stockwell will be obliged by being favoured with the result; and if any person should think favourably of it, and be desirous of a quantity, no doubt it may be had.

*East-India Warehouses,*
*Crutched Friars, 23d May,* 1803.

* Made to the Society of Arts, Adelphi, and printed in Volume XXI. of their Transactions.

*Extract from the Bengal Commercial Consultations, the 24th of April,* 1798.

To W. A. EDMONSTONE, Esq. Secretary to the Board of Trade.

SIR,

I HAVE the honour to acknowledge the receipt of your letter of the 31st ult. informing me that I was desired by the Board to make some experiments with the Chay Root, and to report whether I think it will be of any use to the dyers or calico-printers at home.

Letter to the Bengal Board of Trade.

It is rather surprising, considering the pains that have been taken in Europe to discover, or at least to imitate the method of dying the *Turkish* or *Adrianople* red, that so little attention has been paid to the equally beautiful and permanent red given to their calicoes by the natives of the coast of Coromandel. Although full accounts of the practice of calico printing in the East Indies were sent home long ago by the missionary Cœur Doux, M. Poivre, and others, it does not appear that the European artists have ever tried their skill in the *Chay* root, the drug by which the admired red colour is produced. I have never heard, at least, of any such attempts, nor do I believe that the root has ever been sent home. It is evident, from the manner in which this drug is mentioned by Dr. Bancroft, in his "Experimental Researches," p. 174, that well-informed writer had never seen it, which I think could scarcely have happened, had it been at all known to the London dyers.

The Chay-root affords a beautiful and permanent red to cottons.

Not practically known in Europe.

It is probable that the tediousness of the Indian process, as described by those who sent home the accounts of it, consisting of many tiresome manipulations, continued during a period of nineteen or twenty days, deterred the European artists from trying the effects of the *Chay* root in dying or printing their cottons. What may appear more extraordinary is, that the same cause, co-operating perhaps with the natural indolence of the people, and their having cheaper though inferior red dyes at hand, has prevented the use of this root from obtaining in Bengal; for, so far as I can learn, it is not used in this part of India. The natives here are content with the red produced by the *Aul Munjiet* and other drugs, though the colours yielded by these are far inferior to those of Madras calicoes.

The Indian process apparently tedious.

Cheaper drugs used at Bengal.

On

On this account I am furprifed at the meafure which has been adopted, of fending round hither fuch a large quantity of the *Chay root* from the coaft. I apprehend very little or none of it will be purchafed at the approaching fale, in which cafe it muft be either returned to Madras or fent to England. The expediency of this laft meafure muft depend on the poffibility of fo abridging the procefs of dying, or printing cottons with *Chay root*, that the Englifh artifts may find it worth their while to have recourfe to it for the reds and purples, inftead of the *Smyrna madder*, or whatever other drugs they ufe for thofe purpofes at prefent.

Experiments to fhew whether the Chay root might be of value in Europe.

To afcertain this point, I have, agreeably to the inftructions of the Board, made feveral experiments with the *Chay root*; indeed as many as the fhortnefs of the time, and my other avocations would permit. From the refult of thefe trials, I entertain great hopes that the Englifh calico-printers will not only fhorten the procefs, fo much as to finifh it within a tenth part of the time required for the Indian procefs, but that they will by the *Chay root* dye their cottons of a brighter red than can be done by madder, or any other vegetable.

Cotton was *burred* or galled;

It is needlefs to detail the many trials I made, which either failed altogether, or fucceeded in a very imperfect degree. I fhall therefore only mention that procefs which I found to anfwer beft, and by which the piece of cotton cloth, which I herewith fend you for the infpection of the Board, was printed. Having made a decoction of two ounces of powdered *Hurr*, (the fruit of the Myrobolona Citrona*) in a quart of water, I took a piece of Madras cotton cloth, and boiled it in the decoction for about half an hour.

then wafhed;

Having taken out the cloth, and wafhed it well with cold water, I dried it in the fun, and afterwards had it properly ironed and fmoothed for the pencil.

and a defign made with acetite of alumine;

I then took fome of the acetite of alumine, made in the manner directed by Dr. Bancroft, and, thickening it properly with gum-arabic, I delineated a flower with this mordant upon the cloth, and dried it in the fun.

wafhed,

I afterwards wafhed the cloth in cold water, to clear it of the fuperfluous acetite, and dried it again in the fun. I then infufed about two ounces of the *Chay root*, coarfely powdered, with about a quart of water, in a veffel well tinned, and fetting it on the fire, as foon as the

* Aleppo galls will probably anfwer as well as the Hurr.

liquor

liquor began to grow warm, I put the cloth into it, and let it remain until it had boiled about half an hour, during which the cloth was frequently ſtirred. I then took it out, and having rinced it well with cold water, I put it to dry in the ſun. The delineation of the flower now appeared of a good bright red, and the ground of the cloth, though ſlightly tinged, was much leſs ſo than I expected. By waſhing it again with cold water, and afterwards with ſoap and water, and expoſing it for a whole day to the ſun, during which it was frequently beſprinkled with water, I brought it to the ſtate in which you now ſee it. The whole proceſs has only taken up about ſix and thirty hours. I ſhould have mentioned that the waſhing with ſoap heightened the brightneſs of the red conſiderably.

and boiled in the infuſion of Chay-root.

It gave a good red;

which ſtood waſhing, was improved by ſoap, and cleared by expoſure to the ſun.

Making allowance for my want of experience in the practice of dying, and conſidering the great improvement which may be expected in the proceſs from the ſuperior ſkill of the Engliſh artiſts, I think we may conclude, even from this imperfect eſſay, that the *Chay root* will be a valuable acquiſition to the Engliſh calico-printers. I therefore recommend, that ſo much of the root as may remain unpurchaſed at the ſale, or at leaſt that a part of it may be properly packed up and ſent to England by one of the ſhips now under diſpatch.

This rough eſſay is encouraging.

This drug is the root of a plant called by the botaniſts *Oldenlandia Umbellata*. I have not met with it in this part of India; but it grows naturally on the coaſt of Coromandel, where it is alſo cultivated in great abundance, for the uſe of the dyers and calico-printers.

Some account of the drug.

The ſample which you ſent me appears to be of a good quality, and in good condition. It is ſaid that the root will remain with its virtues entire for ſeveral years, and that they are even improved by keeping. If this be the caſe, and provided the dyers at home find it anſwer, this circumſtance is a very favourable one, and muſt enhance the value of the drug as an article of commerce.

I am, &c.

(Signed) J. FLEMING,
*Inſpector of Drugs.*

*Export Warehouſe,*
*6th April,* 1798.

*Minute*

*Minute of the Board of Trade, dated April* 10, 1798.

Opinion of the Board of Trade, that the root will be acceptable to calico-printers, and of value to the E. I. Company.

THE Board are of opinion, that the refult of the Infpector's experiments will afford very acceptable information to the calico-printers in England; and, if their operations fhould prove his ideas to be well founded, the plant may prove a valuable acquifition to the manufacturers of Great Britain, and alfo an article of commerce from the coaft particularly ufeful, as there is a want of coaft articles of low value, as well light, as a fufficiency of ponderous, to make up a proper cargo for a large fhip, without fwelling its value to too great a rifk, as would be the cafe were a fhip to be loaded entirely with piece goods.

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## XIII.

*Defcription of an Apparatus for raifing Water by the Fall of Wafte Water. By Mr.* SHARPLES, *Portrait Painter, Bath. Communicated by the Author.*

Advantages to be derived from the fall of water.

EVERY drop of water that falls may be confidered as a natural power capable of doing fome work; and it would be productive of confiderable benefit in many domeftic and manufacturing exigencies, if fmall ftreams and currents defcending along the fides of hills, or even through the meadows, were employed, by the application of fimple machinery, to affift the labours of man. I am informed that numerous little mills for grinding corn are diftributed over the face of the country, in Portugal, upon very inconfiderable rivulets; where they not only add to the general product of human work, but may alfo perhaps tend to equalize the price of grain to the confumer. And indeed if it be confidered that a fmall ftream of about a yard wide, running over an obftacle to the depth of an inch, and having fpace to defcend through twelve inches, would do half as much work in the 24 hours as a horfe, whether in grinding corn or turning an engine, or raifing water for ufeful purpofes, we fhall be ftruck with the neceffity of enquiring whether any falling water whatever fhould be fuffered to defcend without due employment in its courfe.

Small mills in Portugal.

A very fmall brook may do work.

The

The ingenious gentleman who has favoured me with the fketch from which Plate XVI. was made, has directed his invention to teach us how the wafte water of our apartments, and other fimilar places, may be made to raife other water in its defcent; and the fimple engine he propofes for that purpofe confifts of the following parts: Engine for employing wafte water.

A, B, and C reprefent three veffels or compartments. The upper compartment A is an open trough, or wafte receptacle, which can be emptied into the fecond clofe veffel B, by turning the cock D; which cock, by the fame motion, opens the communication-pipe I and an air-pipe H, to fuffer the included air to iffue out. N and M are the two legs of a fyphon; the fhorter N, communicating with the refervoir O, containing water, and the longer M, communicating with a trough P, at a lower level which overflows into the drain. The cock F, by the fame turn, opens or fhuts both thefe pipes at once. And laftly, the pipe G forms a communication from the upper part of the receptacle B to the upper part of C. The procefs may be thus explained: Defcription. It is a fyphon with compartments.

When D is opened, the middle veffel B receives wafte water from A. If D be then fhut and F opened, the longer fyphon-leg M admits the wafte water to flow into P; leaving a fpace in B wherein the air in C as well as B expands (with a diminifhed fpring) by means of the pipe G. The preffure of the atmofphere on the water in O will therefore raife that fluid into C, provided the height be not too great; and if the difference between the heights of the veffels B and C be equal to that between O and P, the quantity of frefh water raifed may equal that of the wafte which defcends from the greater height. The foul water runs down one leg, and clean water afcends by the other.

The cock E ferves to draw off the water fo raifed; at which time it is convenient that the cock F fhould be fhut; and this fhould alfo be done whenever the cock D is opened. This may eafily be made to take place by a fimple connection between the cocks, without depending on the fkill or diligence of the fervant.

W. N.

On

## XIV.

*On Gridiron Pendulum Rods composed of Lead and Iron. By Mr.* Benzenberg *.

Mr. B. employs lead, on account of its great dilatability.

MR. B. was induced to employ lead on account of its great dilatability, which is to iron as 2.57 to 1, so that 16.5 inches of lead compensate 1.3 of iron; and he chose iron in preference to steel, because easier to work. The compensation was made by a single rod in the centre, 16½ inches long, French measure, and ½ an inch thick. It was simply pinned into gorges in the cross-piece of copper; but the other parts of the gridiron were rivetted in the usual way. The iron rods were made of the best thick iron wire.

His compensation made by a central rod, 16½ inches by ½ an inch.

Its advantages.

The materials of this pendulum are cheap, and it may be made in a couple of days. As the pressure takes place in a vertical direction, there is no danger, according to Mr. B. of rods of these dimensions bending.

Mode of correcting the compensation.

To correct the compensation, the central rod of lead must be left so long that we may be sure the compensation is in excess. The quantity of error may then be found by the freezing apparatus, and how much it is requisite to cut from the rod may be calculated with the greatest exactness.

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## XV.

*On the Advantages of a large Knife in making Pens. By a Correspondent.*

To Mr. NICHOLSON.

SIR,

The edge of a large knife is better than that of a small one.

USEFUL discoveries are sometimes derived from the common occurrences of life; and that which I am now going to mention, is a mere accident of this kind. About three years ago, I took up a large pocket knife which had been newly ground, and used it for making a pen, and was not a little surprized to find that it answered the purpose much better than

* *Voigt's Magazin fuer den neuesten Zustande de Naturkunde*, Vol. IV. p. 787.

any

any penknife I ever had. I mentioned this circumſtance ſoon after to the cutler who made the knife. He ſaid that he had no doubt of the truth of my obſervation, as he could harden and temper a large blade better than a ſmall one; becauſe there is more difficulty in aſcertaining the proper degree of heat in the latter than in the former.

And it may perhaps be a mechanical truth, that a ſtrong knife will overcome the reſiſtance of a hard quill with leſs exertion of the hand than a weak one. The blade of the knife that I uſe, and which gave riſe to theſe obſervations, is four inches in length and half an inch in breadth. And perhaps the mechanical effect.

It may perhaps be ſuppoſed that a knife of theſe dimenſions is improperly conſtructed for cutting the ſides of a pen into a curve, but it will be found, on trial, that the breadth of the blade is no impediment, for it is the form of its edge which it receives by whetting, that renders a broad blade quite as manageable as a narrow one. It may with convenience and advantage be uſed as a penknife.

I am, SIR,

A conſtant Reader of your

Truly valuable Journal,

*March* 22, 1804. W.

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## XVI.

*Communication reſpecting an Article ſent from the Eaſt Indies, under the Name of Gum Kuteera, and of which a large Quantity has been lately imported into this Kingdom. By Mr.* JOHN COWIE *.

To Mr. TAYLOR.

SIR,

THERE is a gum produced in ſeveral parts of Oude and the adjacent provinces, ſo nearly reſembling gum tragacanth, as to have been taken for it by many; and large quantities of it, of late years, have been imported into Europe, under this miſtaken opinion. But it is now well aſcertained, that this gum (which in the country language is called kuteera) is the produce of a particular tree, of quite a different ſpecies from the Gum reſembling g. tragacanth,

* Society of Arts, Vol. XXI.

thorny

thorny bufh which yields the tragacanth; and being imperfectly foluble, and poffeffing but little of a glutinous nature, renders it inapplicable to the purpofes for which gum tragacanth is ufed. I am neverthelefs of opinion, that this gum might be found ferviceable, in one way or other, to fome of the arts and manufactures, and am therefore induced fhortly to defcribe it, and the ufe to which it is applied in India, with a view of exciting any of your ingenious correfpondents to make experiments on it, and determine its real value, or abfolute inutility.

But not fo glutinous, or applicable to the fame ufes.

Defcription of gum kuteera.

The kuteera is in loofe wrinkled drops or pieces, void of fmell and tafte, of a whitifh colour, and moftly tranfparent. In water, it flowly forms itfelf to a pulp or jelly, and is nearly taftelefs; but, if pounded well in a mortar, and then boiled in water for fifteen minutes, ftirring it all the time, it will be found *completely diffolved.* A tea-fpoonful of this powder gives to three pints of water the confiftence of capillaire. Might it not in this ftate be of fervice to painters and artificial florifts, or even ufed to give a glofs to filks? The natives of India make a varnifh by mixing kuteera with other gums; and I have been told, they likewife make ufe of it in the printing of calicoes. It is the principal ingredient in a medicine they give to their horfes in certain difeafes, and in this refpect is of very general confumption.

It promifes to be ufeful in other refpects.

Large quantities are now in the India warehoufes.

I fhall only add, that many tons of this gum are now lying in the Eaft India Company's warehoufes, totally unfaleable, or that will not fell for more than the firft coft in India; and if this notice fhall be attended with beneficial confequences, I fhall derive great fatisfaction in the reflection of having ferved the intereft of the fpeculators in this drug, amongft whom I could formerly number myfelf; and I believe I was the firft importer of it into this country.

I am, SIR,

Your moft obedient Servant,

JOHN COWIE.

*Finfbury-fquare,*
25th *May*, 1802.

XVII. *Galvan ic*

## XVII.

*Galvanic Experiments with Ice, and a Method of rendering the Electric Attraction of the Pile very evident. By* S. P. Bouvier, *of Jodoigne, Member of the Society of Emulation, of Natural Hiſtory and Phyſics, at Bruſſels* *.

I AVAILED myſelf of the firſt froſts this year to make ſome experiments on ice, both as an interpoſed ſubſtance, and as an exciter and conductor.

*Experiment* 1. A pile was formed of eighty pairs of plates of zinc and ſilver, and as many very thin diſks of ice. It not only did not give the ſlighteſt ſhock, but did not excite the leaſt taſte on the tongue, or produce the ſmalleſt luminous effect. I left it ſtanding for ſeveral hours, but found no difference in the reſult.

A pile of 80 pairs, ſeparated by thin diſks of ice, produced no effect.

*Exp.* 2. I arranged ninety diſks of ice in pairs with as many crown pieces, and placed between the pairs pieces of paſteboard impregnated with a ſolution of ſalt. This combination produced no more effect than the preceding.

A pile of 90 pairs of ice and ſilver, with wet paſteboard interpoſed, had no effect.

*Exp.* 3. The ſame number of diſks of ice were paired with as many of zinc, and wet paſteboards interpoſed as before, with the ſame reſult.

Ice with zinc was equally inactive.

*Exp.* 4. I made a pile of a hundred and twenty-eight pairs of zinc and ſilver, interpoſing paſteboard impregnated with a ſaline ſolution. This pile gave ſuch ſtrong ſhocks, that they extended to the ſhoulders with violence. On forming the circle of this pile with the hands armed with little pieces of ice, all the power of giving a ſhock was intercepted. I then took a piece of ice in my mouth, and brought it into contact with the ſummit of the pile, while with my hand I touched its baſe; but no taſte was perceived on my tongue.

128 pairs of zinc and ſilver, ſeparated by wet paſteboard, gave ſtrong ſhocks felt in the ſhoulders; but if touched with a bit of ice held in the hand, nothing was felt. It could not even excite taſte, when the communication was made with ice.

The thaw prevented my purſuing this inquiry any farther. If the froſt return, I will pair diſks of ſilver with caſt diſks of muriate of lime, of pure potaſh, and of ſulphur of potaſh, in order to produce in the firſt two caſes an action of phyſical ſolution, and in the laſt a chemical action, after the manner of Davy.

Farther experiments propoſed with muriate of lime, pure potaſh, and ſulphuret of potaſh.

* Journal de Chimie, par Van Mons, for January 1803, No. XI. p. 52.

On a pile of 140 feries, a delicate magnetic needle was fufpended; on approaching the hand to it, an ofcillatory motion took place, but the magnetic attraction appeared to be ftronger than the electric.

*Exp.* 5. On a pile of 140 feries of zinc, filver, and pafteboard, impregnated with a folution of falt, I placed an iron pivot, and on this pivot a very delicate and fenfible compafs needle. The friction was almoft nothing. I firft applied one hand to the bafe of the pile, and I carried the other near to the needle. I obferved a flow and ofcillating approach; but the needle was inceffantly invited to refume the magnetic direction, whence I inferred, that the polar attraction exceeded the electric. I tried then to render the tranfmiffion of the fluid more eafy, by making it glide over a metallic conductor, as a brafs wire; but the refult was the fame as when I made the fluid pafs through my arm.

A common pin, fufpended in the fame way, was attracted by a wire communicating with the pile, at the diftance of fome lines.

*Exp.* 6. To the magnetic needle ufed in the preceding experiment, I fubftituted one made of a pin, the movement of which alfo I rendered very free. I approached this needle with the extremity of a brafs wire that communicated with the bafe of the pile, and I perceived it turn with rapidity, at the diftance of a few lines, to rufh againft the wire. A fimilar communication eftablifhed through the arms afforded the fame refult.

A filver thread, fufpended from a knitting-needle, placed on the fummit of the pile, was attracted by the finger.

*Exp.* 7. I placed on the fummit of the pile an iron knitting-needle, bent, and fufpended to it a filver thread, as it is called, ufed for embroidery. I completed the circle with one of my fingers, approaching the thread gradually with it. At a certain diftance the thread applied itfelf to my finger, and remained adhering to it, though it was perfectly dry.

When the communication was made with a brafs wire, vivid fparks were given out, the filver was oxided, and part of it melted.

*Exp.* 8. I repeated the preceding experiment, only forming the communication by means of a brafs wire. The attractive effect equally took place, and very vivid fparks were given out between the wire and the thread, which oxided the latter over all its furface, and melted a part of it feveral lines in length.

The fame effects took place repeatedly.

All thefe experiments were repeated a great number of times, and always with the fame fuccefs.

A filver thread, fufpended from a wire fixed to the fummit of the pile, was attracted by one hand, when the other was dipped

*Exp.* 9. In the upper difk of a pile of 97 pairs of zinc and filver, I fixed an iron wire, and fufpended to it a fimilar filver thread. At the foot of the pile was a communicating bafin, in which was a folution of falt. Dipping one hand into the bafin, and bringing the other near the thread, this immediately came and ftuck to it. When I took my hand out of the bafin, the

the thread immediately quitted the other, but stuck to it again when I redipped my hand into the basin; and so on alternately.

into a basin of salt and water communicating with the pile.

*Exp.* 10. I repeated the preceding experiment, fixing a brass wire, in order to obtain more firmness in the contact with the thread, into a block of lead, placed on the table on one side of the pile. I bent the wire into a crook, and suspended from it an embroidering thread, which I placed at a small distance from the knob of the pile. As often as I held one of my hands wet against the block of lead, and dipped the other into the water in the basin, the thread applied itself to the button of the pile, and stuck there; but it quitted it as soon as I interrupted the communication by withdrawing either hand. This experiment, repeated more than fifty times following, was constantly attended with the same result.

A silver thread, suspended from a wire fixed in a block of lead on the table, was attracted by the knob of the pile, when one hand was dipped into the basin, and the other, wetted, touched the lead; but on removing either hand, the effect ceased.

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## XVIII.

*Experiments on the Activity of a Galvanic Pile, in which thin Strata of Air are substituted instead of the Wet Bodies. By Mr.* Dyckhoff *.

Mr. RITTER has observed †, that all substances which, when interposed between the constituent parts of the pile, excite electricity, are active only so far as they contain moisture; and he asserts, that it is impossible to construct an active pile without the intervention of a wet substance. I take the liberty to oppose to this assertion an experiment, by which it is contradicted. I constructed a pile with disks of copper and zinc, and little bits of thin green glass, about the size of a lentil, three of which I placed triangularly in the intervals that separated the metallic plates. Thus between each pair of metals I had a thin stratum of air instead of a wet substance; and the following were the results:

Ritter asserts, that moisture is necessary in Volta's pile.

But a pile of copper and zinc in pairs, separated by bits of glass, so as to interpose air alone, exhibits electricity;

1. A pile of ten pairs, tried by the condenser, affected the electrometer as powerfully as a common pile of five pairs.

though only in a degree equal to that of a common pile half as high.

* *Voigt's Magazin fuer den neuvesten Zustand der Naturkunde,* Vol. IV. p. 791.

† Intelligenzblatt der allgemeinen Litteraturzeitung, 1802, No. 193.

 2. The

Damp air did not answer so well as dry, apparently.

2. The action remained invariable as long as the air continued dry; but damp air appeared less favourable to the pile.

The electricity was transferrable to the Leyden phial.

3. The same degree of electricity was communicated by the pile to a Leyden phial.

The weakness of the electricity perhaps owing to the roughness of the plates.

The plates were only three inches in diameter. As they had already been used for a pile in which wet substances were interposed, and were much corroded, I took off the oxided portions by a file; but the plates still remained rough and uneven; and to this circumstance I ascribe my not obtaining a higher degree of electricity. I have yet had no opportunity of repeating the experiment on a larger scale; but the result I obtained appeared to me sufficient to render the assertion of Ritter at least questionable. I see no reason why glass, thin strata of sealing wax, or the like, should not have the same effect on Volta's pile as in the condenser. For the experiment to succeed, it is necessary that the bits of glass should be as thin and even as possible; for, if the stratum of air be too thick, it is natural that the electricity, which is excited in such a feeble degree by the contact of the two metals, should remain inactive on the adjacent metallic pairs.

No reason why glass, &c. should not act in the pile, as in the condenser.

The glass must be as thin and even as possible, that the stratum of air may be so.

## XIX.

*Extract from a Letter from Mr.* J. B. VAN DEN SANDE, *Professor at Luxembourg, on the Efflorescence and Decomposition of Glass Tubes* *.

Oxidation of white glass not uncommon.

Old barometer-tubes exposed to flame, turned white and opake;

I HAVE lately had an opportunity of observing at leisure the common phenomenon of the calcination of white glass. I exposed to the flame of an enameller's lamp some old barometer tubes that appeared not to have any defect, in order to bend them into syphons. Scarcely had they come into contact with the flame, when they turned white and opake. One of the tubes being wetted, I placed it on a chamber-stove to dry; and before it was heated, it was covered with a whitish efflorescence. Heating the others produced on them the same effect, I then removed the efflorescence, replaced the tubes on the stove, and they became white afresh. By continuing the experiments,

and the same if simply heated.

* Journal de Chimie, par Van Mons, IV, p. 232.

periments, I reduced an eighth part of the glaſs to a very fine powder; after which the tubes were opake, and had loſt their poliſh.

⅛ of the glaſs thus effervefced.

The greater part of this efflorefcence exhibited in the microſcope rhomboidal parallelograms; the reſt triangles, moſt of which were iſoſceles.

in rhomboidal parallelograms or triangles.

Having poured nitric acid on a portion of the effloreſced matter, it diſſolved 0.20. One part of this ſolution afforded, on evaporation, cryſtals of nitrate of ſoda: the other afforded no precipitate with potaſh.

The effloreſcence was ſoda.

Theſe tubes were become good conductors of the electric fluid; which led me to preſume, that their decompoſition was occaſioned by the humidity of the place where they were kept; which humidity, after it had penetrated the ſurface of the glaſs, imperceptibly inſinuated itſelf into its pores. This efflorefcence, however, depends much on the quality of the glaſs; for we may obſerve in the ſame glaſs frames, with a ſouth or ſouth-weſt expoſure, ſome panes calcined to ſuch a degree as to become entirely opake, while others retain their tranſparency, and are perfectly intact.

Theſe tubes had become good conductors of electricity; the decompoſition therefore owing probably to humidity.

But the effloreſcence depends much on the quality of the glaſs.

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## XX.

*On the Criterions or due Diſcriminations of Cyder Fruit.* By THOMAS SKIP DYOT BUCKNALL, *Eſq.**

To CHARLES TAYLOR, Eſq.

SIR,

HEREWITH you receive a paper on the criterions or due diſcriminations, by which the valuable cyder fruit may be aſcertained, referring to my laſt and preceding Papers on Varieties.

On cyder fruit and the making of cyder.

Report ſays, cyders are not well produced out of the cyder countries. Were it ſaid they are not well made, the aſſertion would be juſt; but it is a miſtake to ſuppoſe that cyder cannot be made in perfection out of thoſe ſituations. Only chooſe the proper ſoil for the fruit, ſee it ground down to a perfect ſmoothneſs, rind, pulp, kernels and all; duly fermented.—Tun the liquor at the exact point of time with regard to ſettling, which is

* Soc. Arts, Vol. XXI.

On cyder fruit and the making of cyder.

is well underſtood in the Weſt, and not attended to elſewhere; and do not rack it too much. Keep the liquor in quantity to a good age; and I take the liberty of maintaining, that there are places in Hertfordſhire capable of producing as good and fine cyder as any in Herefordſhire.

I was once aſked what are the impediments to a more general fine crop of fruit? My reply was, the coldneſs and uncertainty of the climate, injudicious planting, with inattention and neglect in thoſe who have the management of it. As for planting and guarding againſt cold, enough has been ſaid on that ſubject through the whole of my treatiſes. It has been obſerved the cyders were better one hundred years ago than they are now. That may have been the caſe; and I ſhould account for the difference as follows:—When the gentry and ſuperior yeomanry of the country depended on barley and fruits to make an agreeable beverage for their friends, it is natural to ſuppoſe that they perſonally ſuperintended the buſineſs themſelves, and received great ſatisfaction when their labours were attended with ſucceſs; whereas, when Port wines became the luxury of the country, cyder, of courſe, naturally fell into diſuſe at the better tables. Hence the maſters of families were no longer zealous to produce fine potent liquor, and giving it as cyder, that being aſſigned to inferior uſes.

Undoubtedly, where cyder is the general drink of the country, great quantities muſt be raiſed for that purpoſe; and it is done at ſo low and eaſy a rate, as to be ſent from the mill to the neighbours' cellars under three half-pence the gallon. What I am attempting is, to be able to produce, *at a certainty*, a fine generous liquor, ſuch as may do credit to the name of the cyderiſt, and which will depend much more upon attention than is generally imagined.

After having expatiated on the old and new varieties of the valuable cyder fruits, in the laſt volume of the Society's Tranſactions, I have nothing farther to offer for the purpoſe of cloſing the Orchardiſt, than to preſent my beſt reſpects to the Society, and give them the marked criterions as follow:—

My friend, Thomas Andrew Knight, Eſq. has made good diſcriminations in his Tract on the Apple and Pear. The Somerſet report has further extended the ſubject.

I ſay, chooſe an apple naturally ſmall, of a whitiſh colour, ſomewhat tinged with red; of a fine yellow pulp, with a cer-

tain degree of aſtringency, and as much ſaccharine matter as nature is diſpoſed to produce; then obſerve if the cells are large, and full of ripe kernels; and further ſtill, know whether the bloſſoms are patient of cold, and the fruit ripens well. On cyder fruit and the making of cyder.

Such an apple, particularly if a new variety, properly ground down and duly fermented, muſt make good cyder; and both as to profit and uſe, be valuable in any neighbourhood, or as an article of trade. The foundation is now ſo well laid, in the ſeedling beds in the county of Hereford, that within five years there will be more than one hundred new valuable varieties produced. I laſt autumn ſaw two moſt beautiful new apples, of the firſt year's growth, upon the Grange eſtate, and which decidedly obtained the Hereford premium: they were ſeedlings of high promiſe. There is an emulation among gentlemen of that part of the country, which does them great credit.

Dr. Symonds gives ſeveral of theſe diſcriminations; and in addition ſays:—The flavour of a good cyder-fruit cannot be miſtaken by a man converſant with apples, though difficult to be deſcribed; but above all, he recommends to chooſe apples, the rinds of which have follicles, or cells, containing large quantities of eſſential oil, more particularly to be noticed in the old ſtire, golden pippin, and pauſon, and from which Dr. Symonds conceives the cyder in a great meaſure derives its flavour. It is to be obſerved, that the old pauſon, woodcock, and red muſk, are generally large apples. The old ſcorched-harvey is an exception as to the yellow colour; that being white, with a brown ſkin. There are other exceptions; but the material criterions are here enumerated according to the received opinion.

Had I formed theſe diſtinctions when I was a boy, I ſhould then have ſaid, take any apple bordering on the golden rennet or golden ruſſet, and it will make good cyder, in conſequence of ſuppoſing that the golden colour is of ſervice. We had a fine apple, the royal pearmain; it was more flat and large than the Seville orange; it had a thin ſkin and quick taſte, with much ſweetneſs, and made fine cyder: but I preſume that variety is now over, as I have not ſeen one of them for many years.

In the October of 1801, which I ſpent in Kent I was ſhown a new apple. The tree is handſome, and the fruit has moſt of the

On cyder fruit and the making of cyder.

the criterions, but does not ripen well, which is a great defect in cyder making; we named it *The Bland of Hartlip*. It has the macula, or follicles in the skin, more visible than on the golden pippin. I intend examining it with attention at a future period, as it is not possible to form a certain judgment on it for some years. My reason for mentioning the Bland of Hartlip is, to prove that the idea of searching after new varieties has made a general impression throughout the kingdom.

It was the custom formerly, if a new apple had many good points, but ripened late, to preserve the tree as a new variety, imagining that, as it advanced in age and acquired strength, the fruit would ripen the sooner, and consequently the cyder be richer. I should add to it, lay the land dry, and spread plenty of manure. These attentions would much accelerate the wished-for object of improving the liquor, as well as increasing the quantity of fruit.

I remain,

DEAR SIR,

Your obedient Servant,

THO. SKIP DYOT BUCKNALL.

*Hampton-Court, Nov.* 14, 1802.

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## XXI.

*Concerning the Steam Engine from Mr. Blakey, by a Correspondent.* B.

To Mr. NICHOLSON,

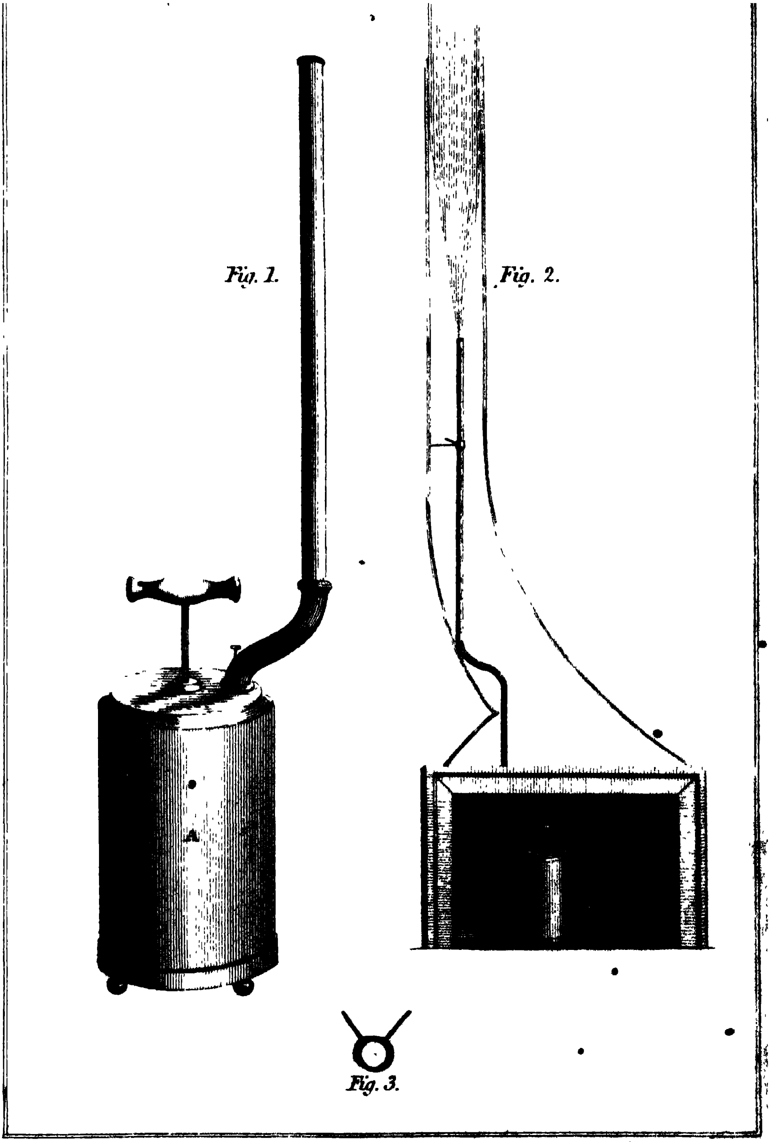
*London, March* 20, 1804.

SIR,

Introductory letter.

SOME time about the year 1776, I met with an engineer of the name of Blakey, in Holland, of whom Ferguson the philosopher and several other ingenious men have spoken in high terms. I do not know whether there is any history of him or of his works; but as many of his contemporaries must still be living, I should hope that this notice may produce some information through the medium of your Journal. I have lately met with a pamphlet by this author, intitled "*A short historical Account of the Invention, Theory, and Practice of Fire Machinery;*" printed at London, in the year 1793, without bookseller's

Apparatus for Cleansing Chimnies,
by Mr. J. C. Hornblower.




Drawn by Blunt. Engraved by Mutlow.

feller's name. It chiefly confifts of fhort notices of the author's own operations, with academical teftimonies; and I underftand from private information, that it was publifhed at that time by Mr. B. in indigent circumftances, with a view to obtain fubfcriptions for a large treatife, which has not fince appeared. Some of your correfpondents may probably favour us with an account of the difpofal of thofe writings fince his deceafe. I have extracted the outline of hiftory of the fteam-engine from the beginning of his pamphlet, which, if you think will be acceptable to your readers, is very much at your fervice.

I remain, Sir,

Your obliged Reader,

R. B.

---

IN the beginning of the feventeenth century, a modern philofopher conceived the notion of making ufe of fire and water to obtain motion. This was Brancas of Rome, who contrived a ftamping-mill to be worked by fteam, coming from a large æolipile, as may be feen in his twenty-fifth plate. We are obliged to him for a number of ingenious inventions, which he dedicated to M. Canci, governor of Loretto, in 1628, and publifhed his work at Rome the year following.

Brancas firft applied Steam to mechanifm; *or propofed it*, 1627.

Thirty-four years after, the Marquis of Worcefter publifhed a little treatife, under the name of "Century of Inventions, printed in London, 1663;" he gave them as if he had tried them, and propofed other engines of great utility, which only wanted encouragement from government: "Several were nothing but projects," fays Defaguliers, "but, where he is more explicit, there is one for raifing water by means of fire."

Marquis of Worcefter, 1663.

In the acts of Leipzig, 1690, p. 410. is to be feen an invention, intitled, "A new manner to procure a confiderable quantity of power with little expence, by Dr. Papin." "In this new engine," he fays, "of which we have given a defcription in the acts of Leipzig, 1688, and the intent of which was to obtain a new method of making ufe of gunpowder as a power to give motion, the chief fault was, that the gun-powder, in the under part of the tube, did not entirely fill

propofed a pifton and condenfation. fill the capacity of this tube, fo as to have no air under the pifton *." Papin continues to give reafons like a man of great underftanding; but as there muft be plates and defcriptions to explain the whole, I fhall fay no more on the fubject, but only add, that the doctor did his beft to obtain his end in another manner. He fhews that, with " A little water changed into fteam by means of fire, we can have an elaftic power like air; but that it totally difappears when chilled, and changes into water; by which means he perceived, that he could contrive a machine in fuch a manner, that, with a little fire, he would be able, at a fmall expence, to have a perfect vacuum, which could not be obtained with gunpowder." Among various contrivances, this appeared to anfwer the beft. In the work itfelf a plate of this contrivance will be given, which, however, is not to be compared to what has been invented fince Papin, by men who had never heard of him, and were altogether much inferior to him in theoretical and practical philofophy.

Capt. Savery, 1699. In 1699, a remarkable year for the honour of Englifh ingenuity, Captain Savery fhewed to the Royal Society an engine which raifed water by means of the expanfion and condenfation of fteam; he publifhed a little treatife, which he named " The Miners' Friend;" the defcription of this engine is to be feen in Harris's Dictionary of Arts, &c. printed in London, 1704. This learned man fays of Savery's engine, " that with little art it can be kept in action as long as one will, and that it cannot be hurt but by ftupidity and negligence." Harris looked on this invention of Savery's to be one of the moft ingenious combinations that had ever been made in hydraulics, and I may add, that it is furprifing that a feafaring gentleman, in all likelihood little ufed to the working of metals, fhould have brought his invention to the perfection he left it in.

* It is feen that Papin is the firft that thought of a pifton to be applied in fire machinery, though it was but in a tube about two inches diameter, yet it fhewed the utility of piftons, which the ancient Greeks knew as well as the moderns, being one of the moft fimple inventions in hydraulics, as may be feen in Vitruvius, fpeaking of the pumps of Ctefibus for the forcing water to a great height.

In

In the memoirs of the Academy of Sciences at Paris, page 112, 1699, is to be feen an account of Mr. Amonton's, who fhewed the drawing of an engine, in which fire heated air, which was to force up water on a wheel, to make it turn. We fuppofe he underftood his theory; but to me it appears impracticable, though it muft be faid that Monfieur Amonton's notions are ingenious and ufeful for thofe who defire to know to what degree the expanfion of air, for procuring power, can be brought. Amonton's rotatory engine, 1699.

There is alfo feen in the fame Academy's works, "Mr. D'Alefme's propofal to raife water by means of fteam, proceeding from hot water; in a machine (a fort of æolipile) in which the elafticity of the fteam makes the water fpout to a great height, fo ftrong is that expanfive power." Auguft 5, 1705. D'Alefme 1705

In 1707, the famous Papin publifhed, at Caffel, a treatife on a new machine for raifing water by means of fire; he thought his manner was better than Savery's. He fays, "All the world fhall know that it is the Landgrave of Heffe to whom this invention is due, and by him it was brought to the perfection it is in, though many obftacles were found in the execution." Papin, 1707.

I cannot help remarking, what a pity it is that fuch an ingenious man as Papin fhould take fo much pains to acquire the reputation of being the firft in the invention of fire-machinery. To appear lefs partial, he takes from his own merit to flatter the landgrave, who certainly did not underftand thofe matters fo well as he: but, notwithftanding the confideration that is due to a fovereign and to the celebrated Papin, I make bold to fay, that this engine was far from having the merit of Savery's; and I can fay more, experience has made me believe that no man has feen the doctor's engine work a couple of hours, as may be conceived from the difficulty he found in making fteam, and encreafing its elaftic power in his receiver. Papin has neverthelefs given us demonftration in upwards of forty pages of calculations, which prove his underftanding and learning. This machine is in my academical works.

In 1710, Thomas Newcomen, ironmonger, and John Cawley, glafier, at Dartmouth, contrived to work a pifton in a cylinder, by the weight of the atmofphere only, when they had formed a vacuum in their veffel; but it took them Newcomen and Cawley's atmofpheric engine, with a pifton, 1710.

much

much time to bring ſuch a complication of apparatus to perfection, for which they were criticiſed more out of jealouſy than reaſon; however, they brought their engine to perform by hand as well as poſſible for that time, much to their honour, as well as to that of a country boy whoſe name was Humphry Potter, and who, to give himſelf time to play, contrived tackles to make the engine work itſelf.

The bringing this engine to perform as it did, proves that, when nature is ingenious, it finds out the way to come to its ends without the humdrum pedantry of ſchools. An ironmonger, a glaſier, and a country boy, brought to perfection the lever fire-engine, which all the univerſities and academies of Europe had not the leaſt notion of before theſe ingenious mortals appeared. This diſcovery has not been improved but in acceſſaries, and by no means as to principles, as theſe are more ſimple, and can work with leſs fuel than thoſe ſo much vaunted, if care is taken in making them.

As ſoon as theſe happy inventions came to light, Beighton, another ingenious man, ſtept forth and made tables for the dimenſions of veſſels, in proportion to the weight of the atmoſphere, which proportions are followed to this day. This has made the world believe that the whole art of fire-machinery is concentered in Newcomen and Cawley's lever-engine.

Deſaguliers, 1718, and Moura, their improvements.

In 1718, Doctor Deſaguliers made an amendment (according to him) on Savery's engine; but his method was not followed, as it cauſed the death of the manager; which muſt often happen when the dimenſions are in the manner he contrived them.

After this engine, Mr. Moura, a Portugueſe gentleman, invented ſome geers to make an engine, on Savery's principles, to work itſelf: they were ingenious, but too complicated to be put in practice.

The making a fire-engine for York-buildings gave an opportunity to improve as to the manual part, and to be leſs expenſive.

Large cylinders,

Experience in making fire-engines has brought Mr. Darby, of Colebrook-dale, to make the cylinders large enough to have a vacuum in them, equal to the weight of 80,000lb. of atmoſphere, ten and fifteen times in a minute. This perfection of workmanſhip has procured to England a great ex-

portation of cylinders, pipes, &c. to many parts of Europe and America. At Liege, and in the Netherlands, they used to have their cylinders from England; but, being very expensive from the duties and carriage, &c. the industrious Posson, at Liege, with his small foundry, found means to make cylinders of three pieces screwed together, which he bored, and made them serve as well as if they were of one piece. I have seen one work as well as can be desired.

One of pieces screwed together.

In 1752, Monsieur Rivatz, a Swiss gentleman, made a fire-engine on Savery's principles for the Chevalier Nugue, at *Nogent sur Marne*, two leagues from Paris; the apparatus to make the engine work itself was of his invention, and performed very well; his boiler, however, being out of proportion, the man who attended the work was much hurt by the explosion of the steam, which discredited the machine, though well contrived in every part except the boiler.

Rivatz, 1752.

☞ *Next follows the Account of the Author's own Works.*

## XXII.

*Observations and Communications on the Dry Rot in Timber, made to the Society for Encouragement of Arts*.*

THE mischief arising in buildings from that decay of the timber and wood-work, known in general by the name of the Dry Rot, has been, and yet continues so great, as to demand every attention for its prevention. In the XIIth Volume of the Transactions of the Society of Arts, published in the year 1794, will be found some valuable facts, furnished by Robert Batson, Esq. of Limehouse, respecting the methods he took to prevent this evil, in one of his rooms greatly affected by it. The plan he adopted was, to charr the ends of his timbers, to take away the infected earth to the depth of two feet, and to fill up that space with anchor-smiths' ashes, or ashes from a foundry, before his flooring-boards were laid. On the 15th of May, 1794, which was upwards of six years after the flooring was laid, as above mentioned, a minute examination of the boards, wainscot, and timbers, was made in the presence of a Com-

Introductory remarks by the Society of Arts.

Robert Batson, Esq. his method of curing the dry rot.

* Extracted from their Transactions, Vol. XXI.

mittee of the Society, and they were all found entirely free from any appearance of the Rot. To investigate the matter more fully, a further inquiry has been made in June, 1803, and an answer received, that there has been yet no appearance of the Dry Rot there; the Society, therefore, think it may be of consequence to notice the fact, and to insert, in the present Volume, some other papers with which they have been favoured upon the subject. They contain many hints deserving public attention, and which will doubtless tend to check the progress of this evil.

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*Letter from* BENJ. JOHNSON, *Esq. on the same Subject.*

To CHARLES TAYLOR, Esq.

SIR,

Account of a pew very much affected by the dry rot.

SOME time between 1771 and 1773, I went, at the request of a friend, to the Chapel at the Lock-Hospital, through curiosity, to examine a pew there, that had frequently been repaired for damages by the dry rot.

It is a plant.

After a close investigation, we found that it was the operation of a plant, whose leaf resembled that of the vine. Wherever it had touched, the effect of its poisonous quality got through the wood to the paint, which I have seen a mere skin. I proposed to cover the floor with bricks, laid in mortar, which was accordingly done. I called twice since, the last time about seven years ago; and have reason to think that it had never appeared again.

Destroyed by covering the ground.

Dry rot at Mark-Hall.

The next opportunity of examining it carefully wat at Mark-Hall, in Essex, the seat of Mr. Montague Burgoyne. In a parlour there were three pillars of about ten inches in diameter, the outwood of which was between two and three inches thick. Two of them were eaten through in less than seven years, from the bases, about two feet upward, within the hollow, and were as rotten as if it had been the effect of a hundred years standing. Mr. Montague Burgoyne's gardener was a botanist: we found the plant where I directed him to search for it; and he said it was the *Boletus Lachrymans*.*

The plant found.

At

* Some authors call it a parasitical plant; and it is sometimes to be found with the willow and sallow tribe, but this is not to the purpose.

At another time, I saw it in a house at Whitehall, built by Sir John Vanbrugh, whose nephew then lived in it. The house is, I think, only two stories high; the plant had ascended to the upper story, committing devastation on the wainscot all the way. It will destroy half-inch deal wainscoting in a year. Another instance at Whitehall,

I have had it twice in houses I inhabited, one in Suffolk, and the other in Gloucestershire. I bore with the first; in the other case, I undertook, and did stop it effectually. and other places.

The cause is from the floor being laid on the earth, which has been, where I have observed, of a gravelly or sandy loam. The moisture from a water-course at hand, or a North aspect, where the outer wall stands in a garden-bed, so that the rain percolates, are great encouragers: it requires moisture. It is caused by communication with moist earth;

It never rises in the middle of the floor; because, if the seed were there, it could not germinate for want of air; but it is easy to suppose, that after the floor is shrunk, an air may be created between that, and the vacancy between the wainscot and outer wall sufficient for the purposes of vegetation. with access of air.

I saw an instance, last summer, in the house of a friend, a student in botany. He was surprised when I told him, it was a visit from a plant; but so it proved, and always is, and ever was so; nor does it originate from any other cause.

In my own case, I removed the original soil near the part affected, and supplied its place with sand. I then placed pieces of tile; on those I laid mortar, and tiles over them, pushing them under the wainscot, so that it had no communication with the joists or floor. Pillars, in like manner, should be kept from the earth. Process of cure.

In laying a floor upon the ground, I should take away the earth for a foot in breadth, and four inches in depth, all round the walls, and place the ends of the joists in mortar, covering them with tiles pressed under the floor and wainscot, quite to the outward wall. Iron or tin plates would do, but are not so cheap as mortar and tiles.

purpose. Till within a few months I have never been without some leaves of the plant. For many years they appear exhausted and dead, and soon crumble into dust; but I suspect that fresh wood attracts a fresh growth from the root.

This

The plant does not run except along wood.

This plant has no adhefive powers, but in contact with wood. If it could pafs over brick or mortar, it might be feen to fpring from the cellars, and infect half the houfes in the kingdom.

In fhort, the wainfcot is to be kept free from contact with the joifts and floor; and I believe it cannot be better effected than I have defcribed.

I am, SIR,

Your obedient Servant,

*Dec.* 21, 1799. BENJAMIN JOHNSON.

To the Secretary.

*(To be continued.)*

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## *SCIENTIFIC NEWS.*

*Society of Agriculture, Commerce, and Arts, at Boulogne-fur-Mer.**

THE following queftions propofed by this fociety are noticed as of general and not local import.

What are the nature of marles, their fpecies and the beft modes of ufing them?

1. It is generally acknowledged, that marling is one of the moft important means of improving land; accordingly the fociety will beftow a prize on the memoir, in which the nature of marles, their various fpecies, and the moft advantageous mode of ufing them, according to the difference of foils, fhall be beft unfolded.

Their external characters neceffary to be known.

The author will be particularly careful to point out to farmers external characters, that will enable them eafily to diftinguifh each fpecies of marle.

Sheep liable to the vertigo or dunt.
In France, 900,000, or 950,000, that is 3 or 4 in 100 die of it yearly.

2. Sheep are fubject to a diforder known by the name of *vertigo*, or the *dunt*. The number that die of this diforder may be eftimated without exaggeration, at three or four in a hundred annually. If then there be about thirty millions in France, we may reckon, that nine hundred, or nine hundred and fifty thoufand are thus loft every year. It is occafioned by a hydatid formed in the head of the fheep. This hydatid

* *Magazin Encyclopédique*, No. 3. June, 1803. p. 401.

contains

contains an extremely limpid water, and a white fubftance, divided as if into grains, which have been found by the microfcope, to be real worms. It appears, that to cure this diforder, the head of the fheep muft neceffarily be opened, and the hydatid extricated: but this operation has hitherto appeared fo hazardous, and has been attended with fo little fuccefs, even in the veterinary fchool at Alfort; that the Society has deemed it highly important to the fcience of agriculture, to offer a prize to the perfon who fhall point out the beft methods of treating the vertigo of fheep, and curing them perfectly of this diforder. He will take care that his method be fupported by experiments made with accuracy, and confirmed by unqueftionable teftimony.

Caufed by a hydatid in the head.

Its extraction dangerous.

What is the beft method of treating it?

The prizes will confift of medals, which the fociety will deliver at the public meeting, on the 28th of April, 1804.

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*Cryftallization of Lime by Tromfdorff, denied by Berthollet.*

IN the firft volume of the octavo feries of this Journal, p. 302, is inferted " A method of cryftallizing lime, by Profeffor Tromfdorff," tranflated from the Journal der Pharmacie, Vol. IX. part I. p. 108. The fame procefs is alfo in Van Mons's Chemical Journal, No. II. According to which, muriate of lime is to be boiled with one fourth or even lefs of cauftic lime, and the fluid is to be concentrated. Afterwards, by flow evaporation, long flender cryftals of *cauftic lime* are obtained, which are to be freed from the adhering muriate by ablution in alcohol.

Error refpecting the cryftallization of lime.

This refult is denied by Berthollet, who, in his *Effai du Statique Chimique, Vol. I. p.* 350, fays, " I have repeated this experiment, and have afcertained that thefe cryftals were *not lime*, as has been announced, but *muriate of lime with an excefs of lime.*" He continues, " if thefe cryftals be treated with water, other proportions are eftablifhed; the part which diffolves is the muriate, which retains but a fmall excefs of lime, and the portion which is not diffolved retains a greater excefs of lime; by adding water, fucceffive feparations may be obtained, and the proportions will depend on the relation of the diffolving force to the refiftance of the cohefion."

Converfion

### *Conversion of Pieces of Cloth impregnated with a Solution of Salt into Soap of Wool, by the Action of the Galvanic Pile.* *By* BRUGNATELLI.

Cloth wetted with salt and water, used in the galvanic pile, is converted into soap of wool; the muriate being decomposed by the zinc, which is oxided, and the soda combining with the wool.

I HAVE examined the singular alteration produced in the bits of cloth wetted with a solution of salt, interposed between the disks of copper and zinc arranged in the pile, and I have found, that they are at length converted entirely into soap of wool.

On this occasion the muriate of soda is decomposed by the zinc, that is continually undergoing oxidation, and the soda, separated by this decomposition, attacks the wool, and forms with it soap.

---

THE Work of Mr. Parkinson, of Hoxton, on the organic remains of the former world, is in considerable forwardness. The first part on the fossils of the vegetable kingdom, illustrated with coloured plates, in quarto, is proposed to be published on the first of June next.

Dr. Herschel's Investigation of the Changes observed in double Stars.

Fig. 1.

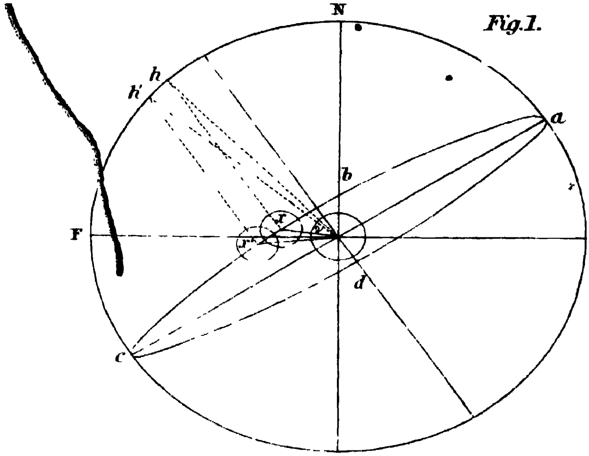
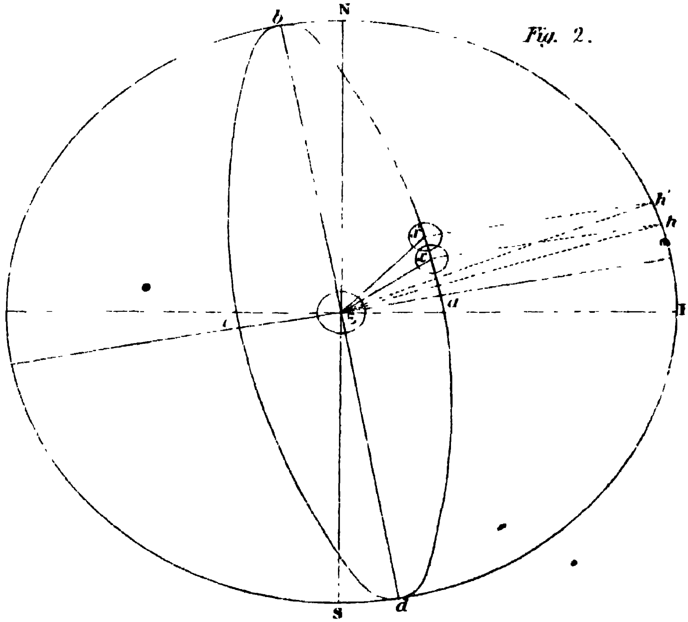
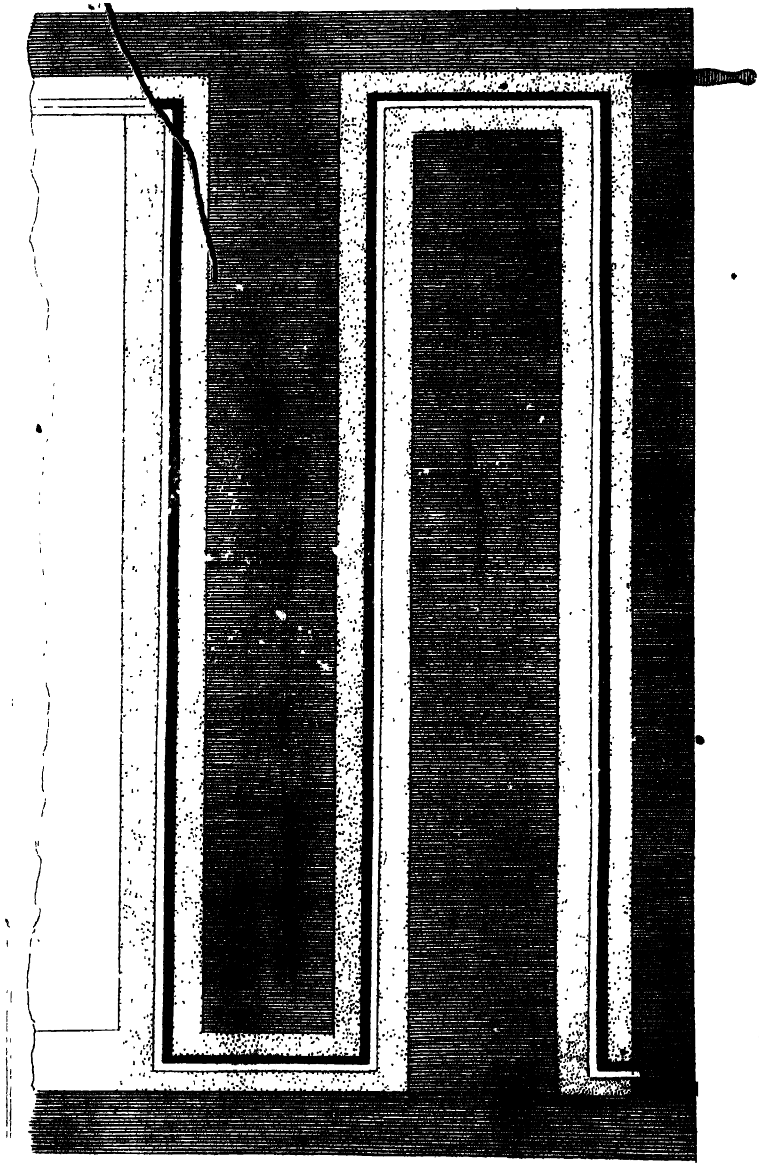



Fig. 2.

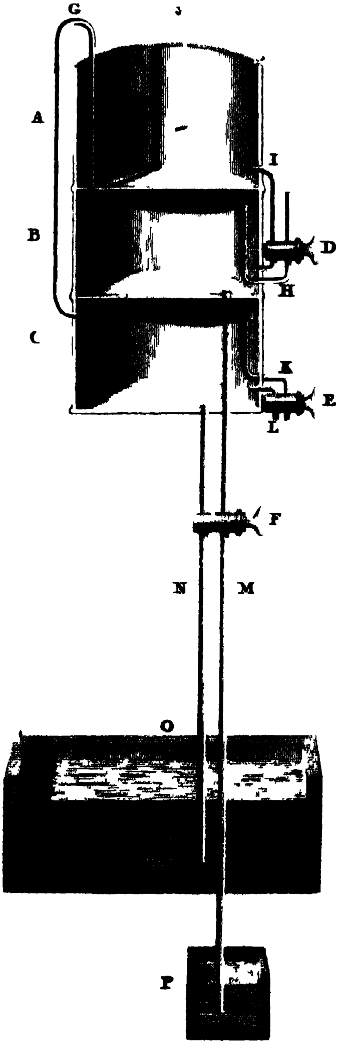



Mutlow sc. Russell Co.

New Galvanic Apparatus for combustion of Metals.



Mr. Sharples' Apparatus for raising Water by the fall of Waste Water.




Drawn by Blunt

Engraved by Mutlow Russell Ct.

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*END OF THE SEVENTH VOLUME.*

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